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PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Contributions to Operative Surgery. Part I.:—Operations about the Face.* By MAURICE HENRY COLLIS, M.B. Univ. Dub.; F.R.C.S.I.; Surgeon to the Meath Hospital and County Dublin Infirmary; Member of Council of the Surgical Society of Ireland, and of the Pathological Society of Dublin; Sometime Examiner in Surgery to the Queen's University in Ireland, and Member of Council of the Royal College of Surgeons in Ireland, &c.

(Continued from Vol. XLII., p. 345.)

IN a standard work on surgical practice, published four years ago, the removal of the tongue is condemned in the following terms:—"The result of ablation of the entire tongue may easily be imagined. So cruel a procedure could hardly have any other than a fatal termination; death occurring, if not from shock and hemorrhage, from œdema of the glottis, pneumonia, erysipelas, or pyemia. As to any ultimate or even temporary good it might produce it is difficult to perceive it. There is a possibility, it is true, that the patient's life might be prolonged for a few days, nay, perhaps, a few weeks or even months; but this would hardly compensate him

for so terrible an ordeal." ^a If the writer of the above sweeping condemnation were now to reconsider his verdict, with a full knowledge of what has been achieved in this direction, he should, perforce, modify his opinion not a little.

The operation cannot now be called cruel; in fact the idea of cruelty in connexion with it is a singular example of the force of old associations. Of itself the operation is milder than multitudes of others which are daily resorted to; especially as all the otherwise painful stages can be gone through under chloroform. In ancient times of semi-barbarism mutilation by ablation of the tongue was a terrible form of punishment. In most bodily tortures it was still permitted to the sufferer to give vent to his misery in cries and exclamations of distress; whereas in this no such pitiful solace was left the victim—lacerated and bleeding, he was popularly supposed to be incapable of uttering a sound; and, as silent agony is ever considered most intense, this punishment came to be viewed as one of the severest which malice could devise and cruelty inflict. To the traditional remembrance of these times of lawless violence we are indebted for the horror with which many surgeons shrink from an operation not specially severe in itself, and in certain cases most merciful in its results.

Ablation of the tongue will be undertaken almost exclusively for epithelioma. I use this term advisedly, for of the existence of scirrhus or encephaloid of the tongue we have no certainty. I have never met with a case of what is commonly called cancer of the tongue which did not commence on the surface. A primary cancerous infiltration unconnected with and unpreceded by ulceration must be very rare.

The description of such a case, as far as my reading goes, is not to be found; nor have I ever met with a museum or *post mortem* specimen of it. Errors of diagnosis have, no doubt, caused syphilitic hardening to be mistaken for cancer, but even in these a sore on the surface is mostly, if not always, found. Now, epithelioma of the tongue is rapid in its progress, as a rule, owing to the favouring moisture of the mouth and vascularity of the tongue. Once it has reached its third stage (of infiltration) the pregnant germs spread themselves with fatal rapidity through the tissues, and in a brief space they pervade the organ in its entire extent. Not only do they cross from one side of the tongue to the other, but they spread

^a Gross' System of Surgery. Philadelphia: 1862. Vol. ii., p. 555.

even more eagerly along the central raphe, where at the line of junction of its two halves the tongue possesses a layer of fine vascular and absorbent tissue, the most favourable nidus possible for the propagation of this disease. Hence it comes that partial removal of the tongue, be it ever so free, is apt to be ineffectual as a means of permanently eradicating the disease. All that is hard may seem removed, and yet enough may be left, unfelt, in this mesial line to perpetuate the mischief. In a few months, or even less, it breaks out afresh, or appears insidiously in the submental or submaxillary region, and the case becomes hopeless. The opinion is daily gaining ground with those who have operated on the tongue that complete removal is a better and more merciful operation than partial, unless in peculiar cases. The benefits of removal of the tongue are not, however, to be measured by the cases which remain free from permanent relapse. These as yet are but few, for no long time has elapsed since the operation came to be considered feasible, and still less, since it became simplified and safe. Independent, however, of complete immunity from relapse, other important advantages accrue. Relief from great distress, and often from extreme pain, greater ability to breathe and swallow, diminution or total cessation of salivary or purulent discharges—offensive and wearing to the patient; finally, strange to say, decided improvement in articulation. These are some of the valuable results, short of cure, which follow the removal of the tongue when affected with epithelioma. Rescue from imminent starvation and great increase of comfort, even for a few months, are results not to be despised, where they can be obtained at an easy rate; and there are ample materials to prove that relief from pain alone, not to speak of the diminished drain on the system, and restored power to eat, are sufficient to ensure a comparatively longer life. For these reasons the removal of the tongue has come to be considered not only a feasible, but what is more important, a justifiable operation; and although but few surgeons have as yet adopted it, the feeling of many is in favour of giving it a more extended trial. My personal experience in the matter is not worth naming as yet. I have operated, one way or another, in four cases, and the results are sufficient to justify the repetition of the operation in similar cases.

Several methods of operating have been practised for removal of the tongue, in whole or in part. The depth at which the root of the tongue lies, its broad attachment to the os hyoides, its tapering form, and the difficulty of seizing it, the still greater difficulty of

passing a ligature round it (especially when distorted by disease), so that the ligature shall not slip—all these combine to render removal of the organ wonderfully embarrassing. Hence the ingenuity of surgeons has been exerted to overcome these obstacles, and the difference of the methods suggested is somewhat remarkable. A short *resumé* of the principal operations may not be out of place, inasmuch as the surgeon of large experience in these cases will have at one time to select one and again another, as most suitable.

Mr. Syme's method is entitled to precedence of notice, both for his eminent position and because of its intrinsic importance. It is a severe and serious operation, and will only be required where the disease extends far back and deeply into the base and root of the tongue—even if then. Mr. Syme did not administer chloroform in his cases, but there seems no reason against its use. I have given it when operating after his method, and in many other surgical proceedings about the mouth and fauces, and I never found any inconvenience in its use; the blood is as easily removed as in a state of consciousness; the patient is able to breathe with perfect freedom, and I need hardly say the operator is much more at his ease. Premising, then, that the patient may be chloroformed, I shall take the description of Mr. Syme's operation from his paper in *The Lancet* of February 4, 1865:—

“Having extracted one of the front incisors, I cut through the middle of the lip, and continued the incision down to the os hyoides; then sawed through the jaw in the same line, and insinuating my finger under the tongue as a guide to the knife, divided the mucous lining of the mouth, together with the attachment of the genio-hyoglossi; while the two halves of the bone were held apart I dissected backwards, and cut through the hyoglossi, along with the mucous membrane covering them, so as to allow the tongue to be pulled forward, and bring into view the situation of the lingual arteries, which were cut and tied, first on one side and then on the other. The process might now have been at once completed had I not feared that the epiglottis might be complicated in the disease (which extended beyond the reach of my finger), and thus suffer injury from the use of the knife, if used without a guide. I therefore cut away about two-thirds of the tongue; and then, being able to reach the os hyoides with my finger, I retained it there while the remaining attachments were divided by the knife in my right hand, close to the bone. Some small arterial branches having been tied,

the edges of the wound were brought together, and retained by silver sutures, except at the lowest part, where the ligatures were allowed to maintain a drain for the discharge of fluids from the cavity."

In Mr. Syme's earlier operation, two in number, death ensued from inflammation of the lungs. This was attributed by him to irritation propagated from the larynx, and led him to modify the operation as originally performed by him. The above description gives, I believe, his latest published account of this mode of operating. In it the os hyoides is not altogether deprived of its muscular supports, the genio-hyoid and mylo-hyoid muscles being left untouched; hence the unrestrained admission of cold air into the lungs is checked, and the larynx is supported. The result was that Mr. Syme's third case recovered. A fourth case upon which he subsequently operated did not, however, end so fortunately, the woman dying of inflammation of the lungs or pyemia.

In Mr. Syme's third case, operated on two years ago, the disease has not returned. "The patient, after recovering from the operation, and while travelling in the highlands, had dined at *table d'hôtes*, and entered into conversation without betraying the deficiency under which he laboured; he could swallow fluids and finely-divided food as well as ever, and could masticate solid substances, although a difficulty was sometimes experienced from their getting into awkward parts of the mouth. In ordinary speech his words were wonderfully clear and distinct, and he could sing without difficulty."

He writes, on December 25th, 1866:—"I am as well as I ever was in my life; in my own opinion I continue to improve in my articulation, and in this opinion I am borne out by many; it is a common remark, when I meet a friend, after not seeing him some time, for him to say, how much you have improved in speaking. I can also say that I have improved much in mastication, and can now enjoy my food better than I have done for some two and a-half years, or more, for whereas I used to eat with pain, I now eat with pleasure."

This result of a case that was in an apparently hopeless state prior to operation is most encouraging.

I have performed Mr. Syme's operation once, for removal of half the tongue. The case is given in the former part of this paper. Should I have occasion again to resort to it, I should adopt a modification of it suggested by Sedillot. It consists in dividing the

bone in a zigzag or notched manner, so that there may be less difficulty in keeping the edges in apposition. This is a valuable hint, as all surgeons will know who have divided the lower jaw for any cause. It is a matter of immense difficulty to retain the edges in immovable contact if they are sawn through smoothly, whereas if their edges are serrated ever so slightly, this difficulty diminishes. By using a narrow-bladed saw, and inclining it a little to the right, until we go half way down through the bone, and then turning it slightly to the left as we complete the section, a sufficient irregularity will be obtained, and we shall find it easy to keep the edges in contact by tying the teeth together with a fine wire, and by the external dressings, without being obliged to bore holes in the bones and tie them together with wire.

Regnoli, of Pavia, made a semilunar incision along the under part of the chin, reaching back almost to the angle of the jaw at either side. This incision extended through the integuments and muscles, and was deepened until the mouth was laid open, when the tongue was seized and drawn through the aperture. If it were too large to admit of this, a vertical incision should be made in the median line down to the os hyoides. The tongue can then be removed either by knife or ecraseur; or a strong ligature may be tightly tied round it, at its base, prior to its removal by the knife, when the vessels can be picked up seriatim, and tied at leisure—the provisional ligature being then removed. This operation, very admirable on the dead subject, or when the tongue is of natural dimensions, would become difficult and inconvenient in cases of extensive disease. It is not free from the subsequent dangers of purulent infiltration and pyemia. I do not know if it have been performed on the living.

In the year 1839, Mr. James Arnott performed an operation by which he was enabled to encircle the root of the tongue with ligatures. He only removed a portion of the organ, but by his operation the entire might be included in the loops of ligatures or ecraseurs. He made a short incision ($1\frac{1}{2}$ inch) in the mesial line, from the os hyoides forwards towards the chin; rooting with the handle of the knife between the muscles he made a space for the finger. Along the finger he passed a strong Liston's needle, and drawing the tongue forward by a tenaculum, he pushed the point of the needle backwards behind the diseased parts, and in front of the epiglottis. The loop of the ligature was caught (not an easy task), drawn forwards, and divided; one half was then, by means of

a curved needle, brought round the root of the tongue, and then out at the wound in the neck. The other half could, in like manner, be passed round the other portion of the root, and the entire tongue be cut off from its basal attachments. Any remaining sublingual attachments could be snipped by scissors or divided by a wire *ecraseur*.

In Mr. Arnott's case ligatures were used, and it required seventeen days to complete the separation. This shows forcibly the value of *ecrasement lineaire*, by which the same end is accomplished in the same number of minutes, and the long dangers of putrid absorption are avoided.

Mr. Arnott may fairly claim the parentage of many of the more modern improvements, of which I shall now proceed to give some account.

To Mr. Nunneley, of Leeds, I think, we are indebted for proving that the greater part of the tongue may be removed by ligature or *ecraseur*, through an incision which scarcely goes beyond a puncture. His operation as at present performed, I take from his paper read before the British Medical Association, at Chester, and published in the Association Journal:—

“I take a sharp-pointed curved blade, about four inches long, and of just sufficient thickness and breadth to carry the wire-rope of the *ecraseur*. This rope I have made somewhat thicker than those ordinarily supplied by Messrs. Weiss, with Hick's instrument; and I always have a second in reserve in case the first one should give way.^a The middle of the rope should be attached by a piece of string to an eye made in its broad end. The patient reclining on his back in a semi-recumbent position, this blade is plunged exactly in the median line, between the base of the jaw and the os hyoides, but somewhat nearer to the latter than to the former, into the mouth, and brought up at the frenum linguæ, and so out of the mouth, the wire rope following. A good sized loop of the rope

^a A certain amount of strength is required, or the rope will break from the resistance of the tongue. While on the one hand it must not be too thick to increase this resistance to too great a degree, so on the other it must not be too thin, or it will act too much as a cutting instrument, and thus give rise to a danger of hemorrhage, to avoid which alone it is employed. I am by no means sure that a chain made of small hexagonal pieces jointed together in alternately opposite directions would not be an improvement. It would make a chain equally flexible in all directions, and be somewhat serrated, by which it would be easily introduced, and when fixed easily cut its way through the parts. One of these chains I am now having made.”

must be drawn through, and the needle cut off. The rope must now be carried well back and spread over the base of the tongue, the tip of which being then drawn through the loop, is seized with Luer's tongue-forceps, and pulled forcibly outwards and somewhat upwards. Two or three long and strong hare-lip pins, somewhat curved towards their points, should next be carefully thrust from the under side of the anterior attachment of the tongue, through its substance, and brought out on its upper surface as near to the base as possible. One of these pins should pass on each side; and if a third be used, it should traverse the median line. Their points should just appear on the upper surface, and over them the rope should be carried. They will thus serve to prevent its slipping forward when it begins to be tightened, as it might otherwise do. They are not absolutely necessary, but I think are useful, and give rise to very little pain; besides which they serve to indicate the exact portion which has to be removed. Of course, the larger this is, the more carefully must the pins be carried well back. The screw of the instrument should now be turned so as to gently fix the wire, that it may not move from the line in which it is intended to cut.

"Hitherto very little pain has been inflicted, and the voluntary efforts of the patient have been useful in facilitating the proceedings; but at this stage he should be put fully under the influence of an anesthetic so that he may not feel, and the screw of the ecraseur be steadily, but very deliberately, turned, the tongue being forcibly extended. It speedily becomes strangulated, and is cut off."

Mr. Nunneley adds, in a letter to myself, that he has operated in six cases, and that all have recovered from the operation without a bad symptom, except one who had diffuse inflammation of the lungs, from which he recovered after a few days; Mr. Nunneley attributed this attack rather to the mode in which the operation was performed, chiefly by ligature, than to any inevitable sequence on removal of the tongue. The small submental wound healed in all by the first intention. Of the results it is satisfactory to learn that one man remained free from relapse for three years, and then died from hereditary phthisis; two died from the development of the disease external to the mouth, and in the abdominal viscera; the other three have been too recently operated on to judge of the amount of permanent benefit. From each and all, however, the lesson can be

drawn that the immediate dangers of Mr. Nunneley's operation are but small, and that for removal of the greater part of the tongue it is an effective proceeding. I am not so satisfied of its being the most efficient operation for removal of disease extending far back or deep into the tongue. It will be seen by his description that the needle comes out at the frenum linguæ, the loop of wire is brought from that back over the tongue, and an oblique section is then made of the tongue. No thought seems to be taken of the sub-lingual attachments to the jaw which form no inconsiderable portion of the tongue, and which are very often the seat of some infiltration. Certainly, in Mr. Nunneley's first case, there was a complete removal of the tongue, but the steps of the operation were different, owing to the chain of the ecraseur breaking, and the necessary substitution of ligature, and the slow process of sloughing. Nor do I see very well what is gained by the submental puncture. If the needles are first fixed in position, the chain, or rope, can be fastened with just as little difficulty from the mouth, without this preliminary puncture; although under no circumstances is this manœuvre very easy of performance. Still it can be done without this puncture, if we use Liston's fixed needles, either straight or curved. The handles of these needles enable us to pass them exactly where we wish; and if we select them with large curves, and strong, they will include most of the tongue, and even of the sub-lingual attachments, and they will guide the ecraseur to the deeper parts of the roots, far back. However, to Mr. Nunneley is due the credit of reducing the cutting operation to a minimum; and, in certain cases, his method may be found the most suitable.

Mr. Paget has operated in a manner which seems to me better than Mr. Nunneley's. He avoids the submental puncture, substituting for it the division of the sub-lingual attachments by the knife. The following description of his operation is taken from *The Medical Times and Gazette* for Feb. 10, 1866. The man having been put under chloroform, his "mouth was drawn open, and firmly fixed so, and the tongue was drawn forward. To facilitate this latter step the mucous membrane and the soft parts on the floor of the mouth, including the attachment of the genio-hyoglossi muscles to the inner side of the symphysis, were cut through close to the bone. The tongue was thus in great measure released from its anterior and inferior attachments, and could readily be drawn forward, so that the wire of an ecraseur was, without difficulty, passed around its root, including the entire organ to its

connexions with the larynx. The wire was tightened by degrees, and there was some free but not dangerous bleeding when the mass was detached.

“The man’s recovery was uninterrupted and very rapid; indeed, he regained his power of swallowing in so short a time that he soon ceased to require special attention in feeding, and before the end of a week he could make himself intelligible by imperfect speech, so much so that it would have been hard, without actual inspection, to have believed that the entire tongue had been removed.”

Mr. Paget writes me that the man returned to his work, that of a butcher, in three weeks, and was active in it for rather more than a year. Then he died with cancer in the sub-maxillary lymphatics.

A second patient, upon whom he operated early in January, 1866, similarly recovered, and has, as yet, had no return of the disease.

He has operated lately in a third case; and this patient, equally with the others, recovered without a bad symptom. Mr. Paget’s observations upon the operation are of much value, and I shall readily be pardoned for quoting them:—

“In remarking upon the operation, Mr. Paget said he believed that what he had just done was suitable in the very great majority of cases in which it was necessary to remove the tongue for cancerous disease. Two points, however, must be attended to in performing it. (1.) The attachments of the tongue to the jaw, both in front, where there were the genio-hyo-glossi muscles, and at the side, where it was connected by the mucous membrane, must be thoroughly divided, so that the organ could be well drawn forward; and (2) care must be taken that the tongue was divided perpendicularly through its thickness; unless there was a definite provision to prevent it, the section would pass obliquely from behind, downwards and forwards, and thus parts that had better be removed might be left. An efficient plan was to transfix the tongue perpendicularly with stout needles at the part at which the division was to be made, and to place the ecraseur-wire behind these, so that it was made to cut vertically. There might, doubtless, occasionally be found cases in which the disease was situated so near the hyoid bone that it would be advisable to perform the larger operation recommended by Mr. Syme. Nevertheless the ecraseur operation allowed of complete removal of the sides and upper part of the tongue, and was, therefore, appropriate in the

greater number of cases. The operation, besides, was attended with scarcely any risk to life."

The following is an extract from a clinical lecture lately given, on the subject of cancer of the tongue, by Mr. Paget:—

"The motive to operate here, as in other cases, is either to prolong life, or, without shortening, to comfort what remains. For the first there is, I believe, some advantage—not a great prolongation of life, yet enough to justify an operation which is attended with very little suffering or risk. But the chief motive is in the hope of comfort; and the comfort that may be gained is, in many cases, so great as to justify a greater risk of life than is incurred in any of the ordinary operations for the removal of cancer of the tongue. The risk is really very small. I have not had a fatal case, or witnessed one—there are few of even the minor operations of which I could say so much—and the comfort given is that the patient is delivered, for the time, from all the misery of one of the most distressing and disabling conditions of disease, and, till the cancerous growth is renewed, may enjoy complete health, and do all his work. Doubtless the disease will return after operation, but it is as unreasonable to refuse a painless operation, and one free from risk of life, because the disease will return at some time soon after it, as it would be to refuse a course of medicine because it gives only temporary relief. When a man has only, suppose, two or three years to live, it is no small advantage if at least half the time can be spent in comfort rather than in misery, and in profitable work rather than in painful idleness. Looking back on the many cases of cancer of the tongue that I have had to do with, I should be disposed to say that there is no organ on which operations for cancer are more justly performed, or are more to be urged, even in extreme cases."

His latest expression of the value of the operation is as follows:—
"I have not yet seen anything to make me believe that the operation will materially lengthen life; but being, as I believe, attended with very little risk, it is justified by the great comfort which it gives. It releases patients from the greater part of the misery belonging to one of the most miserable diseases of which a man can die. And this it does, not only at little risk, but with very slight pain, and so partial a loss of speech that the defect is scarcely noticed by ordinary observers." This strong expression of opinion from a man so calm and judicious as Mr. Paget, must tend largely

to modify the general opinion of surgeons upon this important subject.

In a case, to be related further on, I performed a modification of Mr. Paget's operation, suggested by Mr. Holt, who has used it on two occasions. In place of cutting the sublingual attachments with the knife—which some might be timid of attempting, lest serious hemorrhage should ensue—I divided them by the wire ecraseur; in order to pass the wire it was only necessary to insinuate a curved needle under the tongue as far back as could be reached, and to attach the wire to the loop of the ligature which the needle carried. The remainder of the operation did not differ from Mr. Paget's.

For most cases of epithelioma of the tongue this operation will, probably, suffice; for a few rare and extreme cases Mr. Syme's may, perhaps, still be resorted to; but there are cases in which the disease, without being so extensive as to call for this latter-named procedure, may be situated so far back as not to be within reach by the milder methods of Paget or Nunneley. For these I venture to propose a mode of operation which I adopted four years ago in two cases, and which is described in my work on cancer, published in 1864. By it I was able to go farther back than could possibly be done by any other method short of Mr. Syme's. The ecraseur was worked immediately in front of the epiglottis, and with an ease that was most surprising. The steps of the operation are as follows:—An incision is made through the lower lip, at a distance of a quarter of an inch from the angle of the mouth; this incision passes downwards nearly half an inch, and then turns backwards, and slightly upwards (in a line towards the orifice of the external ear), until it reaches as far back as a point corresponding with the last molar tooth in the lower jaw. This incision goes through all the tissues—first of the lip, and then of the cheek. The object in making the incision at first downwards is to avoid the ugly pucker which results from all incisions that spring directly from the angle of the mouth. This incision gives ample room to enter a strong Liston's needle under the very root of the tongue, and to push it on until the point appears at the other side, close to the bone; a hook or forceps can now be brought to bear upon the loop of the ligature in the eye of the needle, and when it is firmly seized the needle may be withdrawn; the loop is now divided; the chain of an ecraseur is fastened to one of the strands, and is pulled through; it is brought round the base of the tongue, as far back as is desired,

and is slowly tightened; when the screw has been worked for a minute or so, and enough cut through to leave room for a second ecraseur, this is passed by help of the second strand of cord; by its means the sublingual attachments are divided; and, when both ecraseurs are worked home, the tongue is found to be entirely severed from its attachments. It is easy to guide the ecraseurs, and prevent their slipping from what we wish them to cut, by a few strong pins, or by needles in fixed handles; pushing these in here and there we can guide the ecraseur, whether chain or wire-rope, into the line best suited for our purpose. Should it be advisable to remove only a part of the tongue, it is easy to enter the needle as far back as we wish, to push it on to the central line, and then to turn it up, so that the point shall make its exit on the upper surface of the tongue. The needle is best entered while the tip of the tongue is simply raised towards the roof of the mouth; the tongue is then allowed to fall quietly into its place, and the needle is pushed on to the central line, or thereabouts; the tongue is now seized, and drawn forward as far as possible out of the mouth; the needle point can then be directed, so as to make its appearance as far back as is desired. The only difference in the subsequent steps of the operation is, that the ecraseur may be required to cut in three directions instead of two. I have, however, found it possible to remove half the tongue with two ecraseurs by the help of strong guiding needles.

For the removal of the entire tongue, or for removal of any segment which reaches far back, this operation will be found to combine the greatest ease to the operator with the least danger to the patient.

All operations which include section of the bone, or even incisions among the sublingual muscles and fasciæ, are prone to be followed by purulent infection and by inflammatory congestions of the lung. On the other hand it is not easy to get a needle under the tongue far enough back to divide the sublingual attachments efficiently; and until that is done the important ligature round the base of the organ cannot be got into its place. The needle must first be got under and round the base of the tongue before the other attachments can be effectually dealt with, and this can be done most easily by my operation. When the tongue is removed, a few points of interrupted suture will hold the flap of the cheek in its place, and in a few days there will only remain a line of cicatrix, which will scarcely be perceived, in the

male, among the hairs of the whisker, and which, even in the female, will leave but a trifling scar. It is, however, remarkable that epithelial disease of the tongue is comparatively rare in the female—a fact, perhaps, to be attributed to their general avoidance of the fragrant weed.

Mr. Oldham's woodcut gives the outline of Mr. Syme's, Regnoli's, and my own operations, and shows the spot where Mr. Nunneley's needle enters.

The galvano-caustic wire has been used by Dr. Alexander Simpson, of Edinburgh, to remove segments of the tongue; and it is quite possible that occasions may arise in which this method may be found to possess peculiar advantages. I have seen Dr. Bennett use the galvano-caustic for the removal of epithelial disease of the nose; it removes the part rather slowly, but without hemorrhage. However, it labours under a serious disadvantage—it is impossible to be sure that the entire disease is removed. The cautery confounds all tissues, whether sound or diseased, in a similar and undistinguishable eschar. This will, I fear, be a fatal bar to its use, unless in very special cases.

I shall now proceed to detail a case of epithelioma of the tongue, which is remarkable for its slow progress, and in which I removed the entire organ, without using the knife, after the method of Mr. Paget, as modified by Mr. Holt.

Donohue, aged sixty-five, a pensioner, of middle size and healthy aspect, though rather pale. He tells me that six years ago he noticed, for the first time, a small sore on his tongue, of a warty appearance, not painful, except for occasional stings or darts of pain through it. This ulcer lay on the dorsum of the tongue, to the right of the mesial line and about midway between the root and tip. It did not grow appreciably larger for more than three years, when, in consequence of the pain, he had recourse to some persons who applied strong acid caustics to it. From that out it grew larger, but at a slow rate, until last May, when it suddenly increased more rapidly, and continued to grow steadily from that until the date of his admission into the Hospital. It occupied a large irregular space on the right side of the dorsum, and along the margin of the tongue; it had also crossed the central raphe and was extending itself to the left of the mesial line. The ulcer had, properly speaking, hardly any visible floor; it was almost entirely margin. The peculiar mushroom-like margins, which characterize

epithelioma everywhere, are most marked on the tongue, and in this case they so encroached upon the floor of the ulcer as almost to conceal it from view. The outline of these masses was curved or scalloped, showing the coalescence of three or four centres of infiltration. The entire patch might have been covered by one of the larger florins. There was much deep hardness of the tongue as far back as the finger could reach, as also on each side and in front of the ulcer. One of the upper molars pressed upon the centre of the sore; as it was also loose, I extracted it. The disease was epithelial, unusual from its slow rate of growth, and from the persistent freedom from infection of all the glands. There was no trace of syphilis, and no history of anything beyond a simple gonorrhea, twenty years, or more, before. The man, moreover, is married, and has healthy children grown up. I examined him carefully and I had further proof of his freedom from syphilis in the history of his case, as supplied me by Dr. R. Kerans, of Banagher, who sent him up to me. I put him on bark and muriate of mercury, as a further test, but it had no beneficial effect on the sore, and I finally concluded to remove the tongue. As the greater part of the left side was healthy, I wished to save some of it; but subsequent events showed that in this case, as in all epitheliomata, implicating both sides of the tongue, the entire organ must be sacrificed if we are to expect any benefit. The reason of this is not so much from the rapidity with which the infiltration spreads through the muscular structure of the tongue, as from the susceptibility of the fine areolar tissue in the raphe to become diseased. I have found that the disease quickly runs back in the raphe, far beyond the line of infiltration in the muscle. Hence it comes that we often seem to remove all, while there is left behind a most serious centre for propagation of the disease with redoubled virulence. Lymphatic vessels abound in the raphe, and the fine areolar tissue around them becomes quickly poisoned.

Having chloroformed Donohue, on the 17th of October, 1866, I passed a strong curved Liston's needle under the tongue, so as to include as much of the sublingual attachments as possible in the wire ecraseur which I pulled through when withdrawing the needle. The wire was slowly tightened, and worked home; the tongue was now somewhat loosened, and capable of being drawn forwards. I found it impossible to pass a single loop of wire rope round the base of the tongue sufficiently far back to include all the disease. Accordingly I passed a very strong doubled curved needle through

the tongue, entering the point on the dorsum beyond the disease, as I thought, and bringing it out in the sublingual wound. The double ligature which this needle carried was separated and two ecraseur-chains were successively pulled through the tongue. These were tightened, right and left, and the greater part of the right side and more than half of the left were removed. A small suspicious piece in the centre was snipped off with the scissors; and the entire wound was then mopped well with a 30 grain solution of chloride of zinc. The finger did not recognize any disease in the fragment of tongue which was left behind; but in a very few days the man began to perceive that one point in the stump was the source of the old stinging pain; this spot was freely rubbed with the solid chloride of zinc, on more than one occasion, and the man was sent to the country for a couple of weeks to recruit. His articulation was wonderfully good; all the sounds of the alphabet were distinct, with the exception of d, g, h, j, n, and t; and most words could be made out, even from two days after the operation.

At the end of November, Donohue returned from the country recruited in strength. Examination of the stump showed that the left side had healed—the greater part of the right also; but in the centre was a fissure of uncertain depth, running obliquely upwards to the left, and partly hidden by the overhanging fragment of muscle; to the right of this was an ulcer, with mushroom edges, the size of a grain of rice, shallow, flat, and pale; it was the seat of constant pain, as was also the fissure. Accordingly, I proceeded to remove all the remains of the tongue, and by a nearly similar operative measure; when the attachments of the stump to the floor of the mouth had been freely divided by the ecraseur, two strong needles were pushed through the tongue from below upwards and backwards; by the help of these the chain of the ecraseur was kept from slipping forwards, and the entire of the disease was, at length, removed. The chloride of zinc was freely used; one or two vessels which bled slightly were twisted, and the solid chloride of iron was rubbed over the stump to check a little surface-oozing; this it did most effectually; ice was freely supplied.

The surface of the wound looked sloughy next day, and for two days resisted every effort for its improvement. Chloride of soda was freely used, but without effect. I began to fear for the man's health, as his appetite was failing, from the foul taste and smell of the sloughs; however, by painting the stump with an eight-grain solution of permanganate of potash, in water and glycerine, the sloughs

vanished as if by magic; and in twenty-four hours a healthy surface was obtained. Cicatrization went on rapidly, and all is now healed.

In conclusion, I think, we may look upon the ablation of the tongue as an established operation, to be resorted to as a valuable palliative, in cases otherwise destined to suffer fearful misery. It is to be viewed as an occasional method of prolonging life, to a considerable period, or perhaps even of saving it entirely from the ravages of one of the worst diseases. That even partial removal of the tongue may be followed by a very long interval of freedom from relapse, is proved by a case long since recorded by Mr. Tufnell. In this instance after removal of a segment of the tongue by the now exploded method of ligature, upwards of eight years elapsed before death took place from a return of the disease. This result was no doubt exceptionally favourable; but let us suppose for a moment that the disease had, in this case, been left to run its course without a check, and we cannot but conclude that long before the half of these eight years had passed over the patient's head she would have died in misery.

It cannot be too strongly impressed on the mind that the operation is now painless, almost free from risk, and that the relief is immediate and immense. The impairment of articulation is by no means great—nay, it is often diminished; and though death must occur, it is much less painful, and is not preceded by the agonies of a protracted starvation.

I append here a table of such cases of removal of the entire tongue, or of considerable portions of it, as I have found recorded in the journals, and of some which have been communicated to me. I take this opportunity of thanking those who have answered my enquiries on the subject for their courtesy. The table is, no doubt, very imperfect; but it will be, at least, of some assistance towards ascertaining the results of the various operations, and may help as a basis for future and more extended statistics:—

Name	Age	Duration	Cause	Extent of Disease	Glandular Poisoning	Operator	Operation	Date	Result
Sarah K., -	74	9 months,	Smoking,	From base to tip,	Tonsil and palate,	Mr. King, of Hull,	Mr. Syme's older operation,	March, 1862,	Death in nine days from gangrene, pyemia, congestion of lungs.
Sarah V., -	38	20 years,	"	Tumour $8\frac{1}{2}$ inch. $\times 7\frac{1}{2}$, non-cancerous,	"	Mr. Folker, Stafford,	Abscission by knife,	July 8, 1862,	Recovery; tumour weighed $\frac{1}{4}$ lb.
G. S., shoemaker,	47	5 months,	"	All but the very root,	None,	Mr. Syme,	Mr. Syme's older operation,	Dec. 9, 1857,	Death, on seventh day, from inflammation of the lungs.
A man, -	58	6 years, [But had undergone an operation by ligature 4 months back.]	"	Ditto,	None,	Mr. Syme,	Ditto,	July 31, 1858,	Death, on fourth day, from same cause.
Mr. W., -	52	Many years; seriously from 3 years,	"	Entire tongue,	Not stated,	Mr. Syme,	Modified Syme's	Dec. 29, 1864,	Perfectly well at present date (Jan. '67).
Female, -	56	2 years,	"	Nearly all,	Not stated,	Mr. Syme,	Ditto,	May 10, 1865,	Died after 9 days.
James Burke, -	60	1 year,	Wart, going on to epithelioma,	Anterior third,	Not enlarged,	Mr. Wharton,	Ecraseur,	Jan. 6, 1865,	Relapse in a few months in submental glands.
W. C. (male), -	72	14 years,	"	More than anterior half,	Not,	Mr. Holt,	Ecraseur (Holt's modification of Paget's),	March 5, 1866,	No return in tongue as yet.
G. B. (male), -	57	2 years,	"	Right half,	Slight,	Mr. Holt,	Ditto,	Not given,	Died, after 6 months, of cancer of oesophagus.
Rawling, railway guard,	35	Upwards of 12 months,	"	"	Glands not involved,	Mr. Nunneley,	By submental puncture and ecraseur. The entire tongue removed in each case,	Oct. 1, 1861,	Died, 3 years after, from phthisis—mouth quite sound.
Cross, printer, -	54	2½ years,	"	"	Submental glands involved. Had two previous operations,	Mr. Nunneley,	Ditto,	Feb. 29, 1862,	Died, some months afterwards, from internal cancer—probably of liver.
Wm. Steel, painter,	35	12 months,	"	"	Not involved,	Mr. Nunneley,	Ditto,	Oct. 30, 1865,	Now perfectly well.

Name	Age	Duration	Cause	Extent of Disease	Glandular Poisoning	Operator	Operation	Date	Result
Mrs. Macleod, -	44	10 months,	..	..	One gland enlarged and indurated,	Mr. Nunneley,	Ditto,	Jan. 15, 1866,	Much relieved by the operation; died, four months after, from submental cancer; stump of tongue quite well.
Ann Lomas, -	57	18 months,	..	..	Glands not affected,	Mr. Nunneley,	Ditto,	May 4, 1866,	Perfectly well.
Charlotte Thompson,	43	6 months,	..	..	Glands not affected,	Mr. Nunneley,	Ditto,	Nov. 21, 1866,	Perfectly well.
A butcher, -	50	Under 1 year,	..	About three-fourths,	None,	Mr. Paget,	Through mouth by knife and ecuseur,	October, 1865,	More than a year able to work at his trade, then died of disease in submaxillary lymphatics. Still well a year after.
Female, -	49	9 months,	..	Ditto,	None,	Mr. Paget,	Ditto,	Jan. 6, 1866,	
Omnibus driver, -	45	Under 1 year,	..	About half,	Not cancerous,	Mr. Paget,	Ditto,	A month ago,	
Pat. Gallagher, -	50	3 months,	Smoking,	Left half, near the root,	Not engaged,	Mr. Collis,	Two ecraseurs and incision through cheek,	Jan 28, 1863,	Alive in January, 1865; not since seen; supposed to be dead, as disease had appeared in submaxillary glands and rest of tongue.
Darby Sullivan, -	60	..	..	Left half,	Ditto,	Mr. Collis,	Ditto,	May, 1863,	Relapse, August, 1863 — Died.
John Young, -	65	5 months,	..	Right half,	Floor of mouth engaged, Not,	Mr. Collis,	Syme's operation,	June 21, 1865,	Died—pyemia, gangrene of wound, July 5.
Philip Donohoe, -	60	6 years,	Smoking,	Centre and right half,	None,	Mr. Collis,	Holt's modification of Paget's, Screw ligatures (made for the purpose),	Nov. 27, 1865,	Doing well.
A gentleman, -	58	Doubtful,	Cancer, supervening on syph. fissures,	Anterior two-thirds,	None,	Mr. Hutchinson,		1858,	Good recovery. Remained well a year. Died of cancer of glands of neck, without return in the cicatrix.

Name	Age	Duration	Cause	Extent of Disease	Glandular Poisoning	Operator	Operation	Date	Result
Fanny McCartney,	35	12 months,	..	The centre and right side of the tongue only, and not far back,	None,	Mr. Fiddes, of Kingston, Jamaica,	Mr. Syme's older operation—entire tongue removed,	Sept. 2, 1858,	Perfectly sound after 7 months, but died in 12 months from recurrence of the disease.
A man, -	Mid. age,	..	..	Small, limited to right side, and not far back,	..	Mr. Fiddes,	Ditto,	Dec. 1860,	Well in May, 1861; suffering from recurrence in 1863.
..	..	..	..	..	..	Mr. Hutchinson,	Two-thirds of tongue removed,	..	Died, after 18 months, from disease in glands.
J. M'D., -	44	2 years,	Smoker,	Very considerable, with almost fatal hemorrhage,	Not stated,	Dr. Nottingham, (Liverpool)	Settlor's ecraseur,	April 14, 1863,	Discharged, to all appearance well, May 14.
Mrs. M'Millan, -	50	3 months,	..	Ulcer on right side of tongue size of shilling,	None,	Dr. Geo. Buchannan,	Knife,	June 18, 1865,	Rapid relapse, necessitating a second operation.
The same,	..	Relapse,	..	Nearly all right half of tongue,	Right sublingual gland,	Dr. Geo. Buchannan,	Syme's,	Aug. 12, 1865,	Union of jaw in a month; freedom from return up to present date, Jan., 1867.
D. McDonald,	43	2 years,	..	Entire tongue, removed—greater part diseased,	Sublingual	Dr. Geo. Buchannan,	Syme's,	Dec. 11,	Pyæmia on ninth day.
Elizab. Aitchison,	64	1 year,	..	Right half removed,	Not affected,	Pat. Heron Watson,	Three needles and ecraseur, under chloroform	Nov. 1, 1865,	Perfectly well, able to articulate.
A woman,	30	..	..	Right half,	Not affected,	Dr. Reddy, Montreal,	Ecraseur,	May 10, 1858,	Relapse, as under,
The same,	..	Relapse,	..	Left half,	Not affected,	Dr. Reddy,	Ditto,	July 2, 1858,	In good health up to Oct. 1863, when disease appeared in tonsil.

I have been in the habit, of late, of using the solutions of chloride of zinc, sponged freely over the fresh surface in all operations for epithelioma and cancer, as recommended by Mr. Campbell de Morgan. It is yet too soon to judge of the effect of this treatment, which, however, I am disposed to look upon as beneficial. It can do no harm, and not improbably it does good, by destroying lingering fragments or stray germs of disease.

As an example of the occasional benefits which result from partial removal of cancerous tumours, I may here mention a case of cancer of the tonsil, which came under my care in 1862. The man was seventy-five years of age, and otherwise healthy; a tumour had formed on his left tonsil which grew rapidly downwards and inwards. As long as he refrained from the act of swallowing it gave him no special distress, but when the larynx moved upward the point of the tumour came against the rima glottidis. At first, as the pressure was but slight, the irritation was also slight, but soon, as the tumour continued to grow, the difficulty of swallowing became extreme from the spasm which was set up. I removed a large piece of the tumour, which appeared to be of a cancerous nature; the operation was not easy to effect, as the least touch brought on spasmodic efforts to swallow, followed by threatened suffocation. So urgent were the symptoms that I contemplated tracheotomy on the instant; but fortunately I was, at last, able to seize the tumour firmly, and by pushing it forcibly back I kept it from irritating the larynx, and I then removed it. The old man had immediate ease, and a respite of three months, after which time I repeated the operation with similar advantage. After that I lost sight of him, and I suppose he died either of sudden spasm or of extension of the disease to the glands. However, I had the satisfaction of prolonging his life, for a few months, by rescuing him from starvation on the one hand, and spasm of the glottis on the other.

I shall now give an account of a vascular tumour of the tongue in which ligature was used, after a different fashion from that usually adopted when dealing with this organ:—

Case of Vascular Tumour of the Tongue, Cured by a Temporary Ligature.—In the year 1854, a child, named Doyle, aged two years, was brought to me with a vascular tumour of the tongue. It had appeared at birth as a red point at the left side of the tip of the tongue. From this it grew steadily, and when brought to me it had reached the size of a small walnut; it had extended into the

substance of the tongue as far as the mesial line, and backward so as to implicate altogether about one-third of the left half of the tongue. It was blue in colour, with bright red dots here and there on the surface. When compressed firmly it became much smaller, but filled again the moment the fingers were removed. The feel of it was just like a bag of live worms. It had bled violently on one occasion, when the child had bitten it in falling; with much difficulty the parents had stopped the bleeding. They were much alarmed at this, and at its rapid growth. Nevus of the tongue is not very common, and this happened to be the first case I had seen. I treated it as I have often treated nevus of the lip, or elsewhere on the skin. I included the entire growth in two ligatures, tied with some force, so as to compress the larger vessels, and stop the circulation in them, without completely strangulating the mass. In twenty-four hours I cut these ligatures, and removed them. The tumour then shrunk to half its former dimensions, and was little larger than the portion of healthy tongue should have been whose place it occupied. It was firm and solid; a blue grey on the surface, and slimy; with a line of superficial ulceration separating it from the sound parts. Still its heat was good. Next day it had recovered some redness; and, to be brief, in a few day's time the line of ulceration had healed, the tongue became of normal size, and but for the blue colour of the affected part, nothing abnormal would have been noticed. The consolidation of the part advanced from day to day, and I have the satisfaction of knowing, after twelve years, that the cure has been permanent. The boy is now fourteen years of age, and is serving his time in a shop in the Liberties. He has never had hemorrhage from the tongue, nor is it now at all larger than it ought to be; his articulation, as might be supposed, is satisfactory.

I conceive that this method of treating nevi deserves some attention. It is quite practicable to tie a nevus so tightly as to cause its consolidation without necessarily causing it to slough. The ligature can be removed in from twenty to forty-eight hours, according to the density of the integument and the size of the nevus. Small ones will slough off in half the time that a large one will take to consolidate; that is, if they are tied equally tight. It is impossible to give any rule for gauging the tightness in any given case. This is just one of the points where each man must learn by experience. This, however, can be inculcated, that it is better to tie too loosely than too tight, where a mark is of

consequence. A repetition of the ligature is a lesser evil than a large cicatrix.

I may here give details of a singular example of erectile tumour in the lip.

Erectile Tumour of the Lip.—This tumour was not a common nevus, but rather a genuine erectile tumour, composed of a mass of hypertrophied veins and fibro-elastic tissue. It grew, without any assigned cause, on the lower lip of a middle-aged woman, named Honor Reynolds, a native of Arklow. She denied having a habit of biting her lip; nor could she say when she first observed the enlargement. When I saw her, in November, 1864, the lip was about four times as large as it should be, projecting like a large red sausage. It was firm, not very much reduced by pressure, and had no pulsation in it; it was bright red in colour, like the natural lip, and devoid of pain. It caused her no uneasiness other than mental; but she was most urgent to be rid of it. Wishing to spare her looks, I made two transverse incisions, including an elliptical



portion of the lip between them, and removed a wedge-shaped piece from the centre of the growth, I so arranged the lines of incision, and subsequent sutures, that the line of union was hidden. It did not heal kindly, and in two months the lip had grown as large as ever. I had then nothing for it but to extirpate the entire of the parts to which any morbid material extended. Accordingly, on January 20, 1865, I took out a very large V shaped piece, as for epithelioma of the lip, and this time with the result of a complete and permanent cure.

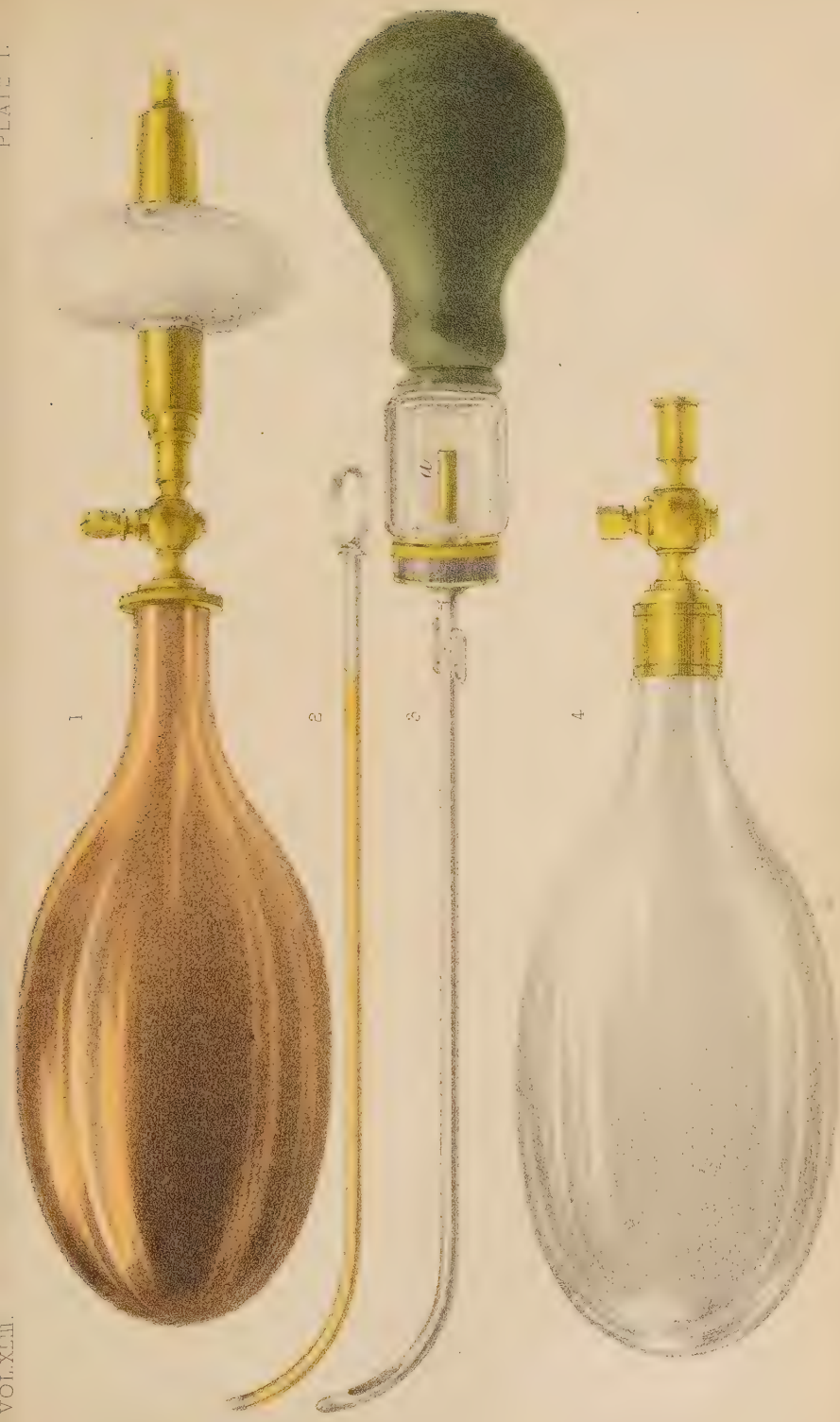
I have met this form of tumour in the groin, prepuce, and scrotum, nymphae, both greater and lesser, on the nose, and, as in this case, on the lip. It is composed of the ordinary elements of skin, with an excess of the venous and elastic tissues in varying proportions. As far as my experience goes, it is liable to return if not wholly extirpated. I have never seen it degenerate into any worse form of growth; but there is no reason why it may not be sometimes a nidus for cancer or epithelioma. When it is inclined to grow it is certainly best abolished.

ART. II.—*Lithotrity, and its After-treatment.* By GEORGE H. PORTER, M.D., Univ. Dub.; F.R.C.S.I.; Senior Surgeon to the Meath Hospital and County of Dublin Infirmary; Surgeon to Simpson's Hospital; Examiner in Surgery Royal College of Surgeons, Ireland; Consulting Surgeon to the Coombe Lying-in Hospital; Member of Council of the Surgical Society of Ireland, and of the Pathological Society of Dublin; Lecturer on Clinical Surgery.

CASE I.—PATIENT, AGED SIXTY-THREE YEARS; LITHIC ACID CALCULUS, OF SMALL SIZE; LITHOTRITY; RECOVERY.

CASE II.—PATIENT, AGED FIFTY-NINE YEARS; LITHATE CALCULUS, OF MEDIUM SIZE; LITHOTRITY; RECOVERY.

It will be freely acknowledged that the most brilliant surgical operation proves unsuccessful occasionally, owing to the after-treatment. The many difficulties with which the surgeon has to contend, more or less excuse him for consequences, many of which, very probably, the peculiarities or disobedience of his patient will best explain. Perhaps there is no operation which bears out this statement more forcibly than lithotrity, which is in itself difficult to perform properly, but still is one whose favourable termination may be hoped for more from the judicious subsequent care of the case, than from the most expert manipulative skill during the performance of this triumph in surgery. A stone having been fully crushed in the bladder, it becomes one of the most important parts of the after-treatment of the case to remove all the fragments.



And, as the process of disintegration must be so frequently repeated, lithotrity may, with great propriety, be regarded as a series of operations, each operation or sitting being but a step towards the desired end.

In the February number of this Journal for 1866 I published some observations on crushing stone in the bladder, together with three cases in which this operation had been successfully performed. I am now about to add two additional cases; and in reference to these latter to make some remarks upon a point in the after-treatment of cases of lithotrity, further experience having led me to modify my opinion as expressed on the subject in my former paper. The point to which I allude, is the removal of detritus by washing out the bladder.

CASE I.—A gentleman, aged sixty-three years, put himself under my care on the 11th July, 1866. He had suffered much from irritability of his bladder during the preceding three months, and for the last three weeks of the time he had been greatly annoyed with pain along the under part of the penis, near the glans. He had passed some very small calculi, and occasionally crystals of lithic acid. My friend, Dr. Ledwich, had previously sounded him, and detected the presence of a stone. His urethra was large, freely admitting a No. 13 metallic bougie, and its introduction proved that the canal was not irritable. His prostate was natural, and his general health had not apparently suffered. Assisted by Dr. Ledwich, I placed him lying on a sofa, and introduced Mr. Henry Thompson's flat-bladed lithotrite, for the purpose of measuring the calculus. I was fortunate enough to seize it almost immediately, and ascertained that the diameter in which it was caught measured half an inch. I then screwed home the male blade, and crushed the stone, and shortly afterwards I caught a small fragment, and broke it.

July 13th.—Second sitting. I caught and broke three fragments of small dimensions, and washed out fifteen grains of detritus with Mr. Clover's syringe.

July 17th.—Third sitting. Two fragments crushed, and six grains of detritus removed by Clover's syringe.

July 20th.—Fourth sitting. Four small pieces were broken, and a large fragment was washed out by means of Clover's apparatus.

August 1st.—Fifth sitting. One small piece was seized and broken, some fine detritus removed with Clover's syringe.

It would be useless to note the daily progress of this gentleman's

case. He improved rapidly, all symptoms of his complaint disappeared, and Dr. Ledwich and I were unable to detect any unbroken fragments by careful sounding on the 20th of August. He is now in the enjoyment of excellent health and spirits, and takes a great deal of exercise, frequently driving on an outside car, as much as twenty miles a day, over rough country roads.

Between the sittings detritus came away during micturition, which, together with that brought out in the jaws of the lithotrite and removed by means of Clover's instrument, amounted to seventy-nine grains. In all those cases some portions of the stone must have been lost, owing to the difficulty of separating the fine debris from the urine, and inducing attendants to carefully preserve this fluid for the surgeon's inspection.

CASE II.—A gentleman, aged fifty-nine years, consulted me in August, 1866. He had been suffering from an irritable condition of his bladder for seven years previously, and for the last two years the symptoms assumed a serious and painful character. He was obliged to pass water very frequently, both by day and night. The pain, whilst expelling the last drops of urine, was most severe, and he voided it bloody whenever he either walked a distance, or took carriage exercise.

He had taken a variety of medicines intended to relieve his distressing state, but without any very decided benefit. He had suffered from retention of urine twice within a short time before he came under my care. His health had not given way as much as might have been expected. His urine was highly acid, as ascertained by my friend, Dr. P. C. Little, who saw this gentleman along with me; he also carefully tested it for albumen, and found only a trace, which could be explained by the presence of blood corpuscles. On the 29th of August I sounded him with Thompson's sound, and detected the stone. It measured six-eighths of an inch in diameter.

His urethra was large, and pervious, but rather irritable, from the frequent passage of bougies before I saw him. His prostate gland was of healthy dimensions. He had expelled some small calculi from time to time, two of which, about the size of duck shot, he gave me; their composition, on analysis, was found to be lithate of ammonia.

September 2nd.—Assisted by Dr. Little, I proceeded to crush the stone. I placed the patient lying on his bed, with a pillow under

the nates, and his head comfortably supported with other pillows. I had desired him to keep his urine for two hours previously; but as he was unable to do so, I injected four ounces of tepid water into his bladder. I then introduced Mr. Henry Thompson's flat-bladed lithotrite with ease, quickly seized the calculus, and broke it. Five fragments were subsequently caught, and crushed. Having withdrawn the lithotrite, and introduced Clover's catheter, I attached its syringe, filled with tepid water, and washed out ten grains of detritus. I then gave the patient a glass of sherry, and placed a quarter grain morphine suppository in his rectum; he was put comfortably in bed, with a jar of hot water to his feet.

September 6th.—The second sitting took place. Between this date and the first operation, he passed some fine detritus. On this occasion three fragments were seized, and broken; one of them measured half an inch in diameter. Clover's instrument was also employed, and five grains of detritus removed.

September 12th.—The third sitting. Prior to this sitting he had expelled one large fragment, *per urethram*, by the natural powers.

The fourth sitting took place on the 18th of September. He requested me to administer chloroform, as he felt very nervous, and dreaded the lithotrite, not because he had suffered pain to any great extent, but he felt depressed, and wished for the anesthetic. He was rendered insensible, most carefully and efficiently, by my friend, Dr. Cruise. On this occasion I seized four pieces, and crushed them, and immediately afterwards washed out twelve grains of detritus with Clover's syringe. In the jaws of the lithotrite, also, a good deal of pulverized calculus came away. In the interim between this and the next sitting several fragments of small dimensions passed off.

September 24th.—The fifth sitting. He was again chloroformed for me by Dr. Little. I caught and broke five fragments, and used Clover's syringe with the effect of removing nineteen grains of detritus, some large pieces having come away by this mode of washing out the bladder. I have not detailed the various symptoms from day to day in this case, as such would only weary the reader. Suffice it to say, after this operation he progressed rapidly; all vesical irritation subsided; his health vastly improved, he was able to take several drives into the country, and to retain his water for hours.

Sixth, and last, sitting took place on October 12th, when he was

again chloroformed. I introduced the lithotrite; but the most careful examination of the bladder failed to discover any fragment of the calculus remaining unpulverized, and but two grains of detritus were sucked out with Clover's syringe; the whole quantity removed weighed one hundred grains.

In both the cases just detailed, it will be perceived that I used the flat-bladed lithotrite of Mr. Henry Thompson. Its power is quite sufficient for medium-sized calculi, and its beautiful construction renders it very safe while feeling for, and breaking the stone. The medical treatment consisted in the administration of a liberal allowance of wine, with plenty of beef tea; and, as a general drink, oatmeal tea, which I have found in those cases more soothing to the urinary passages than decoction of linseed, barley, or gum-water. Whenever mucus appeared in the urine after a sitting, I ordered a mixture, containing infusion of buchu with tincture of hyoscyamus and liquor potassæ; and occasionally, small doses of balsam of copaiba. I introduced a quarter-grain morphine suppository into the rectum after each sitting, and also whenever urethral or vesical irritability existed, with the most marked benefit.

It is evident that as soon as a stone within the bladder has been reduced to fragments, it is most desirable that those fragments should be removed as soon as possible. For those who do not employ the scoop, following the practice of Sir William Fergusson, there remain but two means of effecting this object; one method, deprecating interference with the bladder as much as possible, leaves the fragments to the unaided contractile force of the viscus, trusting they may be expelled with the urine in due time; the other plan, and more trustworthy one, endeavours to promote their removal by washing out the bladder with a strongly-injected stream of water, expecting the return current to carry out much of the detritus stirred up in the bladder. It must, however, have been observed by practical surgeons how often the amount of detritus removed in this way has disappointed their expectations; and it was the failure of the means which I then had at my disposal to remove detritus, which obliged me in my last paper on lithotrity, to place little value on the practice of washing out the bladder. The water thrown in with the ordinary evacuating catheter and syringe returns with but *little* force, particularly in cases in which the tone of the organ has been in a great measure weakened or lost. It has always been a desideratum to supplement the force of the bladder in the

removal of detritus. The late Sir Philip Crampton saw the necessity of some efficient means of doing so, and not only suggested but employed a most ingenious contrivance, which, in my student days, I saw him use in the Meath Hospital, and which is well described in the number of this Journal for January, 1846, page 22, in the following words:—"The apparatus consists of a strong glass vessel of an oval form, and six or eight inches in length, by three in diameter, and capable of holding about a pint and a-half of water; to this vessel is attached a tube of about half an inch bore, furnished with a stop-cock. The air being exhausted by means of an exhausting syringe, and one of Heurteloup's wide-eyed steel evacuating catheters being introduced into the bladder, it is next attached to the exhausted vessel; the stop-cock is then turned, and a communication being thus established between the bladder and the glass, the pressure of the atmosphere is by this means brought to bear on the bladder, and supplies an expulsive power, which may be increased to any required amount." Figure 4, plate I, gives a tolerably accurate idea of this instrument. It has been drawn from the *original* one, which my colleague, Mr. P. C. Smyly, kindly placed at my disposal for that purpose.

The instrument of Mr. Clover, of London, for injection of the bladder, and withdrawal of the injected liquid by *suction*, is such an improvement upon the former means of removing debris by injection, that having found the advantages its employment affords in speedy and safe removal of the fragments of calculi, I consider its use an important auxiliary in the after-treatment of lithotrity. It must be admitted that there can be no more important element in the after-management of a case of lithotrity, or a point more conducive to the success of the operation, than early removal of the detritus, and without injury to the prostate or urethra; and improvements in the surgical appliances for lithotrity are to be much encouraged in this direction as well as in the means of primary comminution of the calculus. By crushing the stone, in the first instance, a comparatively smooth and single calculus is converted into several angular and irregular stones; the bladder, moreover, having become more or less irritated by instrumental interference, the patient is not much better off than he was before the operation. His hopes of relief, therefore, rather depend on the removal of the crushed fragments. Every practical lithotritist will bear me out in saying that the delight of a patient is most remarkable when he sees the fragments; and, on the contrary, his

spirits are depressed when the debris is tardy in making its appearance. I have found the instrument devised by Mr. Clover (to whom the profession is also indebted for an apparatus for the safe administration of chloroform) most efficacious in removing any fragments sufficiently reduced in size to pass through the large catheter attached to his syringe. For those who may not have had an opportunity of seeing it, I have added a drawing, Plate I., Fig. 3; and in explaining it I think I can best do so by quoting Mr. Clover's own description, as given in his letter in *The Lancet* for May 12th, 1866. He says:—"My instrument consists of a glass cylinder two inches long and two inches wide, having an eight-ounce India rubber ball at one end, and a vulcanite mount at the other, in which is a hole which fits closely to a collar fixed just above the rings of the catheter, so as to allow the catheter to project three quarters of an inch inside the glass vessel."

The instrument that I have been in the habit of using was procured for me from Weiss, of London, by Messrs. Fannin, of Grafton-street. It differs from, but possesses advantages over, that described by Mr. Clover, namely, the mount is of brass attached to the glass, and to it is *screwed* on the stopper, which has a tube, Fig. 3, the calibre of a No. 12 catheter, fixed to it, and extending into the cylinder about three quarters of an inch. This arrangement renders it unnecessary for the catheter to project into the glass portion of the instrument, and allows the apparatus to be attached to, and detached from the catheter with greater ease.

It has been proposed by my friend Mr. Fleming to improve the instrument by adding an offset to the glass part, resembling that of the ordinary breast-pump—*vide Dublin Quarterly Journal of Medical Science* for February, 1866, and by Mr. Maunders—*vide London Hospital Reports*, Vol. III., 1866, page 216. The late Sir Philip Crampton was not unmindful of this addition to his instrument, as will be seen. Plate I., Fig. 1, shows a glass receptacle attached to the brass tube united to a copper cylinder, which he had constructed to bear greater exhaustion than the glass one. I confess that, although ingenious, I cannot see the advantages of such an offset to the glass portion of Mr. Clover's syringe. When the end of the catheter extends sufficiently into the cylinder it renders it most difficult, if not impossible, to force back any fragment that may have been sucked into the instrument. Indeed, in practice I have found that a good deal of debris remains in the

India rubber portion of the apparatus instead of the glass, and certainly if it ever passes beyond the projecting portion of the catheter into the glass it will be retained just as well as with the offset. To the practical surgeon I consider this instrument of Mr. Clover's a great boon, not only to remove detritus after lithotrity, for which he suggested it, but also for the removal of clots of blood from the bladder, as I myself lately found it most efficient in a case of bleeding into this organ, arising from diseased prostate. The catheter should be of the size of No. 12, with a large eye in the concavity of its curve, and provided with a stilette made of gum-elastic (Plate I., Fig. 2), to fill this aperture during its introduction and withdrawal.

The fragments of the calculus removed by lithotrity in the first case were analyzed by my friend Dr. William Daniel Moore, who stated that it was composed of lithic acid. It is unusual, at an advanced period of life, to find a calculus wholly or so largely composed of lithic acid, although in some or other of its salts it is one of the most usual ingredients of all calculi. But while there is no age exempt from the occurrence of urinary concretions, into which lithic acid enters largely, they are, perhaps, more associated with the urinary diseases of earlier than of later life. With this form of calculus there are certain advantages in the performance of lithotrity; there is less apprehension of delay and tediousness in the secondary crushings, because the absence of phosphatic salts indicates an integrity of the mucous membrane of the bladder, and an acid rather than an alkaline condition of the urine, both of which circumstances very much promote the success of any operation implicating the interior of the bladder. In cases where there is reason to believe lithic acid, or its salts, to be undergoing deposition in the bladder, it is dangerous to render the urine alkaline in the hopes of interrupting the formation of a calculus, on account of the tendency which the phosphates exhibit of forming in layers round lithic acid calculi. As soon as the urine is made alkaline, although the deposition of lithic acid may be checked, a layer of triple phosphate will replace it, and the calculus will increase in size as rapidly, if not more so, than before. In the after-treatment of cases of lithic acid calculus it is of importance to prevent a re-deposition in the bladder of this ingredient of the urinary excretion, which will be very liable to occur if the diathesis be not combated. With regard to the urine, it is not necessary to render it more than neutral, for which purpose the waters of Vichy

and the carbonate of lithia are well adapted. It is to be borne in mind that should it be rendered alkaline the phosphates are ever ready to take advantage of a small hemp-seed or pin-head particle of gravel, and upon it, as a nucleus, to initiate a rapidly-growing stone. The object of keeping the urine neutral is to keep the lithic acid in a soluble condition until after its removal from the body. It is also most important to keep the action of the skin healthy, as it has been ascertained that the quantity of lithic acid in the urine is augmented when the cutaneous transpiration is interfered with. Warm clothing, the wearing of flannel or chamois leather next the skin, occasional diaphoretics, and the use of the vapour bath, are means of warding off the excessive formation and deposition of lithic acid concretions which will be worthy of attention after the removal of a calculus of this description. At the same time it will be necessary to endeavour to restore the proper tone of the organs of digestion, and to combat any peculiar form of dyspepsia which may be present, since the lithic acid diathesis is so much due to defective primary assimilation.

ART. III.—*On the Theory and Principles of the Ophthalmoscope.*

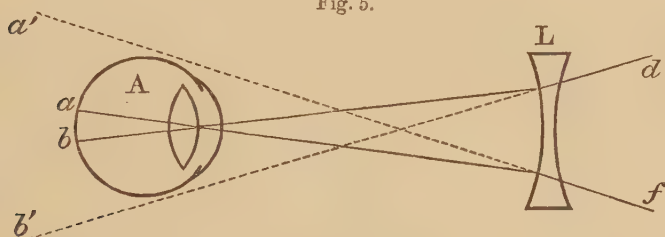
By HENRY WILSON, F.R.C.S.I.; L.K.&Q.C.P.I.; Assistant Surgeon to St. Mark's Ophthalmic Hospital; Member of the Royal Irish Academy, &c., &c.

(Continued from Vol. XLII., page 360.)

IN the production of the *erect* image we employ the ophthalmoscope mirror alone, and make use of the refracting media of the eye itself as a lens; in this examination the observed as well as the observer's eye must be accommodated for distance, and be very close to each other—about two inches; the emergent rays being parallel, will be united on the examiner's retina, and form there an inverted image of the observed fundus; this image, which is magnified by the dioptric apparatus of the observed eye itself, and apparently situate behind the eye, will, however, be mentally perceived as upright. Another mode of producing the erect image consists in the employment of a concave lens, as shown in the

accompanying illustration. If $a\ b$, Fig. 5, be two points in the illumined fundus of A , pencils of rays will emerge parallel to the axial lines $a\ L$, $b\ L$ to form at the anterior focal distance of A an image of $a\ b$. The concave lens L , however, being now inter-

Fig. 5.



posed, these rays are diverged to d and f , and they appear to proceed from the prolongations of these lines—from $a'\ b'$, at which place an enlarged *upright image* of $a\ b$ will apparently be situated. The direct examination is principally of value in cases where it is desirable to obtain a more accurate observation of the minute details, such as individual vessels or nerve fibres, or in the determination of anomalies of refraction, such as myopia or hypermetropia. It is, however, attended with more difficulties than the indirect method of examination; it necessitates, also, a very close approximation of patient and examiner, which may, for various reasons, be undesirable or objectionable; the enlargement is very considerable, and therefore only a small portion of the fundus can be seen at a time; moreover, the concave mirrors of the ordinary ophthalmoscope do not suffice for this examination. I would, therefore, recommend beginners first to employ the indirect method of examination; and as soon as they may have become familiar with this method, and the reversed image, they may proceed to the direct examination.

As might naturally be expected, there is a great variety in the form and construction of ophthalmoscopes; but no matter how they may apparently differ in outward shape and appearance, they are, one and all calculated to fulfil the same purpose—to illumine the background of the eye by dispersed light, and to allow of the parts so illumined being clearly seen at a convenient position and of a suitable size. The essential part of the ophthalmoscope is the perforated mirror; this may be either of glass, backed with a layer of amalgam, or of metal; the metal specula are preferable to the glass ones, inasmuch as they are less liable to injury, but

chiefly on account of the central aperture through which the observer looks; in the glass mirrors this orifice is more or less in the shape of a tube or canal, from the internal surface and edges of which there is very likely to be most disturbing and annoying reflection of light, which would obviously interfere with the reflected light from the fundus; in the metal mirrors, on the contrary, the walls of the central perforation are extremely thin and the margins sharp, and there can be, therefore, very little, if any, reflection from this part. The illumination afforded by glass mirrors is, moreover, in my opinion, too intense (provided the examiner's eye be normal); the details can be discerned more satisfactorily by a somewhat subdued illumination; hence I am in the habit, when using a glass mirror, of breathing on its surface, or otherwise damping its reflection. When I was in Berlin, some years ago, Liebreich, who has since then removed to Paris, employed a metallic mirror which he had dulled by means of sand-paper. All mirrors have necessarily an opening, and are usually attached to a suitable handle or frame.

Ophthalmoscopes may be divided into two great classes—in one the mirror is concave, and its focus calculated from its surface definite and fixed; such instruments are termed *homocentric*. In the other class the mirror is either plane or convex, and in combination with a double convex lens; in these the focus may be altered, is negative, and situate behind the surface of the mirror; such instruments are denominated *heterocentric*.

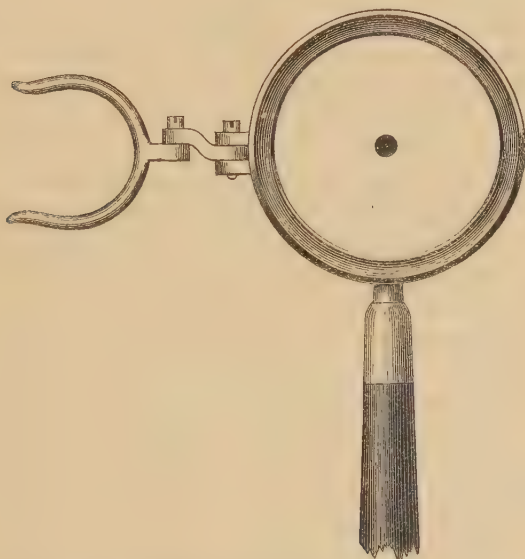
An enumeration and explanation of the several individual instruments constructed would be of little practical value. I will, therefore, merely adduce a few of the more important ones, commencing with the *homocentric*.

The simplest, most convenient, and, at the same time, most practical of all instruments is Liebreich's small or hand ophthalmoscope, and is the one I usually employ. It consists, as shown (the actual size) in Fig. 6, of a concave metallic mirror $1\frac{1}{4}$ " in diameter, and of 8" focal length,^a which is fixed into a metallic frame, to which the handle is attached. The central aperture is a shallow, funnel-shaped opening, the base $\frac{3}{8}$ " wide in the metallic frame, the apex nearly $\frac{1}{8}$ " wide in the mirror itself; the sight-hole thus enlarging gradually from before backwards, and the parts adjoining it, and indeed the whole back and sides of the instrument being

^a The inch is, for brevity, expressed by ("), the foot by ('), and the line by (').

thoroughly and effectually blackened there can be no reflection from these parts and no interference with the rays coming to the observer's eye. The handle, about $3\frac{1}{2}''$ long, is screwed into the frame, to which is also attached a clip with a jointed movable arm;

Fig. 6.



the clip serves to hold a small convex or concave ocular lens, which can thus be brought immediately behind the opening in the mirror, and consequently in front of the examiner's eye. The instrument is fitted into a case $5\frac{1}{4}''$ by $2\frac{1}{4}''$, and little more than $\frac{1}{2}''$ thick; two large double convex object lenses, generally of $1\frac{1}{2}''$ and $2''$ focus; four small concave ocular lenses, of 6, 8, 10, and 12 inch focus, and one small convex ocular lens of $10''$ focus are likewise fitted into the case. In my own case I have substituted for the $2''$ convex lens one of $3\frac{1}{2}''$, as I find in the general run of cases the enlargement produced by the latter much more satisfactory. One of the uses of the small ocular lenses is to neutralize any error of refraction or accommodation which may exist in the examiner; another use is to effect the examination in the erect image; the convex ocular may also be employed to magnify the reversed image. The large lens, whether convex or concave, which is held before the examined eye in order to produce the image, is styled the *object lens*, to distinguish it from the small one placed in the clip behind the mirror, which is called

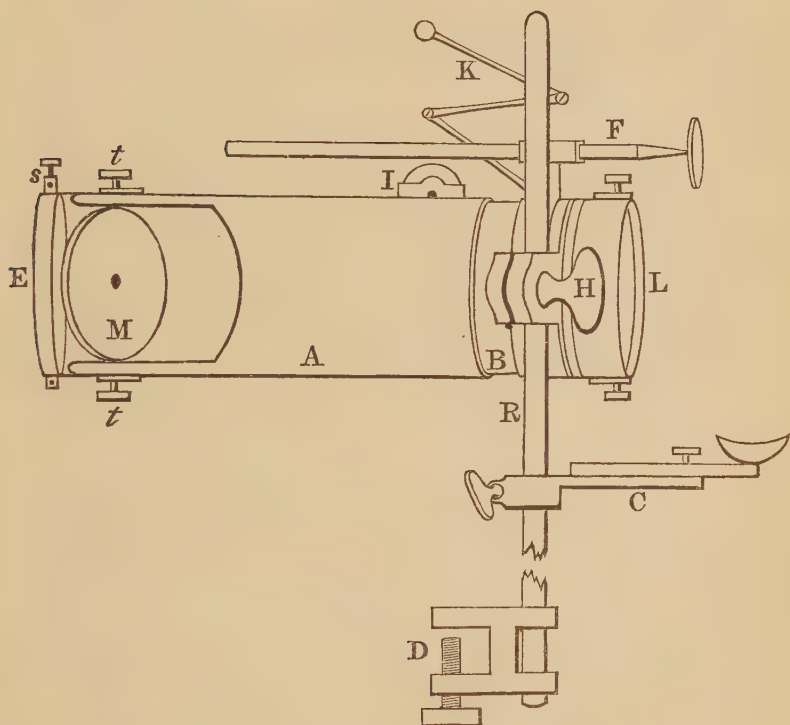
the *ocular lens*, and the reflecting surface of the ophthalmoscope is regarded as the front of the instrument. Dr. N. C. Macnamara, professor of ophthalmic medicine and surgery in Calcutta, has lately published a most admirable and trustworthy work on ophthalmoscopy under the title *Lectures on Diseases of the Eye*; but in his description of Liebreich's instrument he states that "attached to the mirror is a clamp by which one of the *object-glasses*, to be found in the instrument case, may be fixed *in front* of the sight hole." This sentence, which to a beginner would be so confusing, shows the necessity for a strict adherence to a uniform nomenclature.

Messrs. Weiss manufacture an ophthalmoscope consisting simply of a concave mirror set in a circular frame, the principal recommendation of which is its portability, as it may, together with a convex lens, be carried in the waistcoat pocket or in a pocket instrument case.

The best of the *fixed ophthalmoscopes* belong to this division; such instruments are so constructed that when adjusted they and the patient's eye shall retain a definite, fixed, relative position, so that once the image is projected a number of persons may in succession, examine it without any trouble or difficulty on their part. Here, likewise, Liebreich's ophthalmoscope is undoubtedly the best. The accompanying diagram, Fig. 7, (about one-third the actual size), shows its construction. A and B are two tubes moving one within the other by means of a rack and pinion, I, and having their interior well blackened. At the end of the tube A a concave perforated mirror, M, is swung on trunnions, *t t*—and a portion of the walls of the tube cut away so as to allow the rays from a laterally placed lamp to fall on the mirror; the tube B carries the convex object lens L, of 2" focus, swung, like the mirror, on trunnions: this tube is provided with a stout encircling collar, having at one side a projecting clamp, capable of being widened or narrowed by means of the screw H; an upright rod, R, is affixed to the corner of a table by means of a strong clamp, D, and the instrument is then affixed to this rod by means of the perforated projection, and slides up and down on the rod, and may be fixed at any wished-for height by the screw H; on this rod slides also a stout horizontal jointed arm, C, having at its extremity an oval padded cup, which acts as chin-rest; this, of course, is put on the supporting rod beneath the ophthalmoscope. A horizontal bar, F, slides in a box projecting from the upper part of the collar: at the extremity of

this bar is a padded disk, which serves as a head-rest; a small jointed movable arm, K, carries a polished metal ball, for directing the patient's eye to any given position, and a shade, projecting laterally from each extremity of the instrument, at s and B, screens the observed and observer's eye from the lamp; there is also a clip behind

Fig. 7.



the mirror, M, for holding a convex lens, three of which are contained in the case. When in use the patient's chin rests in the cup, C, and his forehead against the pad, F; the instrument should usually be about two inches from the eye—the focal distance of the lens L, and a lamp placed towards the side, from which rays may fall on M, and so directed then through L into the eye; it will be seen that the anterior extremity remains stationary, while the tube A, carrying the mirror, is movable. The formation and position of the image is the same as that already described and illustrated by Fig. 4. When obtained, the image of the fundus is very clear

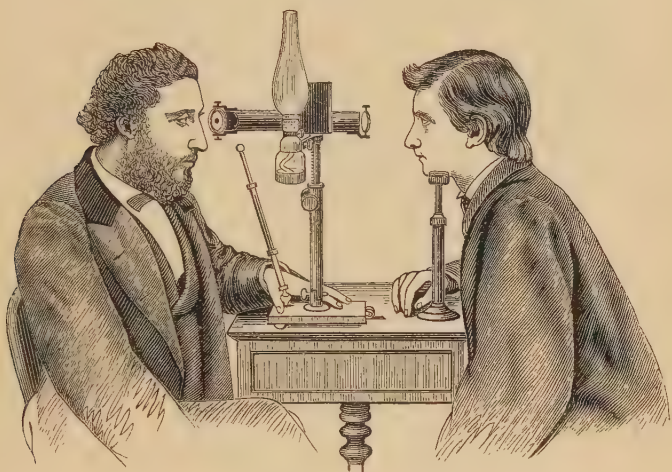
and beautiful, and may be readily seen by any number of persons in succession. A camera lucida for drawing purposes may be attached, and also a microscope; its inventor has also constructed an ingenious addition to the instrument, by means of which the fundus of the living eye may be photographed. Professor Follin, whose learned and well-written *Leçons sur l'Exploration de l'Œil* I can confidently recommend to those who are familiar with the French language, has also constructed a fixed ophthalmoscope; but in its optical arrangement, and even in its mechanical construction it appears to differ but slightly from the one just described. I have found the manipulation of Liebreich's instrument rather difficult, and its adjustment often tedious and troublesome, the light being one of the principal sources of trouble; I had a lamp made some years ago which I attached to the upright rod, and which I found to answer much better than the lamp placed beside the instrument; the instrument, too, is liable to shake or tremble from the nature of its support. These and other circumstances led Messrs. Smith and Beck, the eminent opticians and microscope manufacturers, of 31, Cornhill, London, to construct a modification of Liebreich's large ophthalmoscope. This instrument^a is shown in use in Fig. 8.^b It consists, like its prototype, of two tubes, one carrying the lens, the other the mirror. These tubes are, however, fixed permanently on an upright stem capable of being raised or lowered by a rack and pinion; to this stem is attached a horizontal arm which carries the lamp. No matter how the instrument may be moved, up or down or from side to side, the flame of the lamp and the mirror will always be on the same level, and always retain their relative positions; the instrument is mounted on a broad board, having three small rollers fixed in its inferior surface, so that it can be freely moved about on a table or any smooth surface; by means of a half-rule joint, secured by a pin close to the base, the board is capable of being turned up against the stem, and the lamp, being brought round to the mirror end, the instrument may be fitted into a small space. As seen in the engraving, a chin-rest supports and steadies the patient's head at any convenient height; a small shade screens the light from the patient's eye, and a small ivory ball at the end of a sliding tube, with a ball and socket joint at its base, serves for fixing the patient's eye at any wished-for position.

^a Already figured, and described by Dr. Wright in the Number of this Journal for August, 1865.

^b From a photograph by Allen, 12, Westland-row.

I now employ this in preference to Liebreich's original ophthalmoscope, as it is much more easily and quickly adjusted, and affords an equally good image; even an inexperienced person may, by its

Fig. 8.



aid, see the fundus of an ordinary eye without any difficulty. I have been able also on several occasions to demonstrate satisfactorily the fundus of my own eye to others by means of this instrument.

Another useful tubular ophthalmoscope, although not nearly so efficient or practical as the two just described, is that constructed by my friend Dr. Galezowski of Paris. This instrument is not exactly a fixed one, but holds an intermediate place; the ocular end of the tube projects beyond the object lens, and is so shaped and padded as to fit over the margin of the patient's orbit, and so to exclude all light except that transmitted by the ophthalmoscope mirror; the instrument can be shortened or lengthened in the same manner as a telescope.

The principal advantage of fixed instruments is, that by their means the fundus can be satisfactorily demonstrated in a short time to a large number of observers. Fixed ophthalmoscopes are, however, at all times cumbrous and do not afford a satisfactory view of the peripheral portions of the fundus; hence they are, and probably will always remain, merely ancillary to the hand ophthalmoscopes.

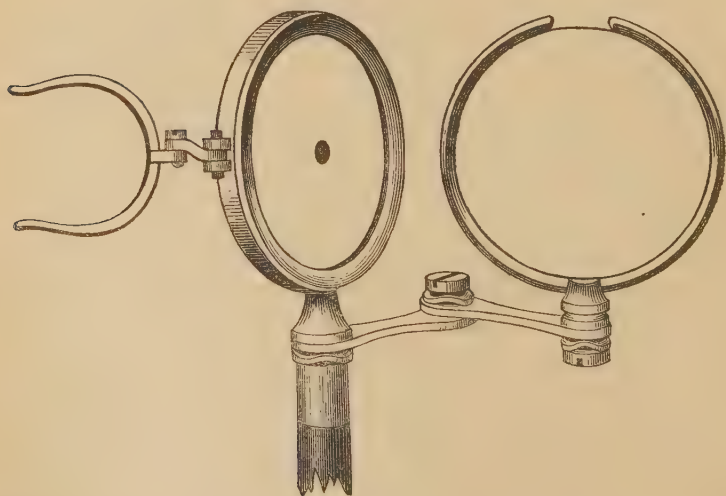
Ophthalmoscopes in which the mirror is either plane or convex,

and in combination with a double convex lens, are termed *hetero-centric*, and were first devised by Coccius.

The convex lens, which must be of shorter focal length than the negative focus of the convex mirror, is interposed between the flame and the mirror. These ophthalmoscopes are based on the principle of Brücke's experiment, already alluded to (Fig. 2)—*i.e.*, of the eye under observation being accommodated for a different distance than that from which the light actually emanates. Coccius' ophthalmoscope consists of a plane perforated mirror fixed to a handle, attached to which is a jointed arm carrying an upright clip with a convex lens in it: there is a smaller clip on the mirror frame for an ocular lens. Figure 9 may be supposed to be an illustration of this instrument. The rays of light passing through the convex lens fall on the mirror convergently and are reflected by the latter converging into the eye, and form on its fundus a circle of dispersed light. The eye regards the bright surface of the mirror, but in consequence of the action of the convex lens the image of the flame seen in the mirror does not correspond with an actual flame in the position of the reflector, but with one placed a certain distance behind the mirror.

Zehender's ophthalmoscope, shown, the actual size, in Fig. 9,

Fig. 9.



differs from Coccius' only in the mirror, which is convex; in appearance, therefore, these two ophthalmoscopes are at first sight identical.

The lens is $1\frac{1}{2}$ " focus, and the convex mirror has a curvature of 6" radius. This is a most valuable instrument, and preferable to Coccia's; it affords a larger and better field of illumination than any other ophthalmoscope, and is especially of use in the direct examination. Its manipulation is, however, not very simple, and hence partly it is not such a favourite as Liebreich's hand ophthalmoscope. The light must be at the patient's side, and the lens inclined at such an angle that the rays shall traverse it, and fall on the mirror, from which they are reflected into the eye.

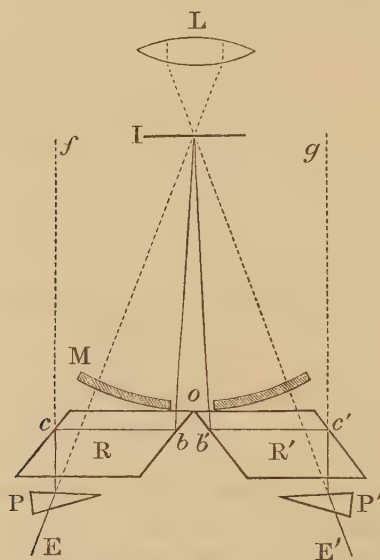
Two other forms of heterocentric ophthalmoscopes deserve mention, but more for their ingenuity than for their practical utility. Ulrich employed two prisms presenting in section the forms of right-angled triangles, with equal containing sides, and fixed them together in such a manner that the hypotenuse of one should be at right angles with that of the other; rays of light falling on the first prism reach its hypotenuse, and are there totally reflected and thus projected into the eye, the fundus of which they illumine; emerging from the eye they strike the hypotenuse of the second prism, and are reflected by it at right angles into the observer's eye.

Another form of ophthalmoscope is that of Hasner, and consists of a convex or concave lens, having one surface silvered, with the exception of a small space in the centre, which acts as a sight-hole. The chief object aimed at in these is the combination in *one* of the reflector and the lens; a biconvex silvered lens acts as a concave mirror—a biconcave silvered lens as a spherical mirror.

The construction of the instruments I have mentioned, and the large variety of kindred ophthalmoscopes, is such that the observer can become cognizant with *one* of his eyes only; hence the errors and deceptions which arise from monocular vision are at all times likely to occur. To rectify this defect Dr. Giraud-Teulon constructed an ophthalmoscope, by means of which binocular vision was secured, and an image of the fundus obtained, in which the parts are seen as they actually exist—in relief, or depressed, or plane. This *binocular ophthalmoscope* is constructed on the principle previously applied in microscopes by Mr. Wenham, and by Nachet, the optician, of Paris—that of employing prisms or rhombs for the purpose of deflecting the rays in part or in whole. Giraud-Teulon employed a glass concave mirror, with a large oval opening in its silvering, and placed behind it two rhombs of crown glass, each representing a double prism of 45° , and capable of effecting total reflection from their inclined surfaces; these rhombs were

placed horizontally behind the mirror, with their apices meeting behind the orifice. The accompanying diagram will serve to explain the nature and principles of this instrument. *M* represents the concave mirror and *L* the convex lens, by means of which the reversed aerial image *I* is formed as with the ordinary ophthalmoscopes; if we take any point in this image we find rays proceeding from it divergently; thus, a pencil of rays is represented in Fig. 10 passing through the orifice *o* in the mirror. Here it meets the

Fig. 10.

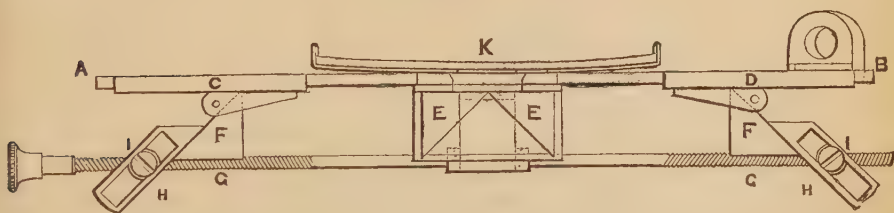


rhombs *R* and *R'*, and divides right and left; the ray on the left side is reflected at *b* to *c*, where it is again reflected to *P*, and emerges from the rhomb; in the same manner the ray on the right side undergoes total double reflection. If an observer now place his eyes in the line of emergence of these rays at *E* and *E'*, he will see two images, *f* and *g*; by placing two prisms, *P*, *P'*, however, with their bases turned outwards behind the rhombs, the emergent rays are deflected outwards and receive the direction *E I*, *E' I*; the two images are fused into one, and thus both eyes participate in the visual act. As is evident, the width between these emergent rays must correspond to the distance between the examiner's eyes, for otherwise one eye would be excluded from seeing the image, and the instrument would be simply a monocular one. Nachet subsequently

remedied this defect by dividing the right-hand rhomb into two, and making the outer half of it movable, so that the instrument as now made can be adapted to the distance between the eyes of various observers. The rhombs are enclosed in a metal case, in which are spaces for the prisms, four of which are supplied, of various angles; there is an aperture at one side of the frame, into which a handle may be screwed; I frequently use the instrument, however, without the handle, holding it between finger and thumb. The mirror is attached by a universal joint, so that the instrument can be used with the flame either above or to the side of the patient.

My friend Mr. Zachariah Laurence, ophthalmic surgeon to St. Bartholomew's Hospital, Chatham, and the Ophthalmic Hospital, Southwark, has devised a very ingenious modification of this ophthalmoscope; it consists of two reflecting prisms, E, E, placed

Fig. 11.



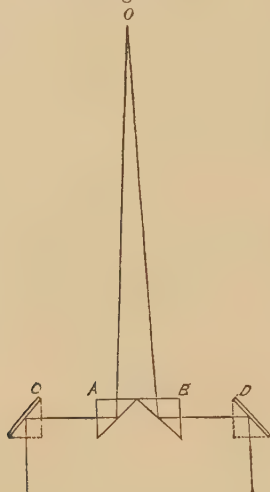
behind the opening in the mirror, K, and two others, F, F, termed oculars, at some distance to the outside; these latter "shift upon an horizontal metallic plate, A B, and can be adjusted to any required inclination by means of a screw G G, carrying the nuts I I, which work in the slots, H H."

"The optical action of the instrument is represented in Fig. 12. O A and O B are the extreme outer rays of a pencil, proceeding from a point (O) of the inverted image formed by the ordinary object lens; the ray O B is reflected by the prism B to the prism D, and hence to the observer's right eye placed behind D. Similarly, the ray O A is reflected to the observer's left eye. He then sees *two* images of the fundus oculi. By inclining the ocular prisms (D and C) inwards by the mechanism described above, the two images are fused into *one*." ^a I am indebted to Mr. Laurence for the loan of these two cuts.

^a Laurence and Moon's Handy-Book of Ophthalmic Surgery.

The framework and adjusting mechanism is, however, fragile and delicate, and easily injured by careless handling; it is said, however, to prevent the squinting which may occur in the examination

Fig. 12.



with the French instrument, and to produce a clearer image; and its weight is only half that of the other. I cannot speak with confidence on the relative merits of the two, as my experience of Laurence's has been somewhat limited. I have, however, found its manipulation and adjustment difficult, and prefer Nachet's improvement of Giraud-Teulon's ophthalmoscope as a more practical instrument for every-day use; many, however, prefer Mr. Laurence's to Giraud-Teulon's binocular instrument. The manipulation of the French instrument is so easy, and the image afforded by it so beautiful, that I am now beginning to employ this binocular ophthalmoscope in preference even to Liebreich's hand ophthalmoscope. These ophthalmoscopes are most useful in the examination of cases in which an inequality of surface may be suspected; thus, in glaucoma they afford a beautiful and clear image of the cup-like depression of the optic nerve and of the vessels on the sides and in the bottom of the depression.

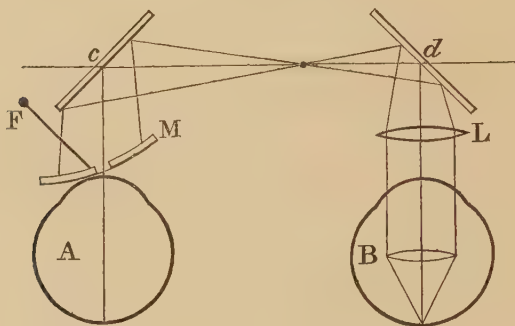
As is evident, the image of the fundus can be examined only by one individual at a time. Mr. Z. Laurence, has here also brought his great knowledge of the science to bear on the subject, and in 1863 proposed a method whereby the original aerial image may, while

being regarded by the observer, be reflected so as to become visible to a *second* person at the same time; he interposed at right angles between the ordinary reversed image and the ophthalmoscope mirror a plate of glass, with plane parallel surfaces like that used in Helmholtz's first ophthalmoscope; by means of this plate a duplicate image is formed, which may be seen by a second person placed in a certain given position. Giraud-Teulon has suggested an addition whereby even a *third* person may participate in the examination; this is the employment of a plane perforated mirror to reflect the light on to the ophthalmoscope mirror; a lamp is placed near the ophthalmoscope, and is furnished with a chimney, blackened on three-fourths of its circumference; through the remaining clear portion the light proceeds, and falls on the plane perforated mirror, and is reflected by it on to the concave ophthalmoscope mirror, from whence it proceeds to illumine the fundus; the original observer sees the image in the usual way through the ophthalmoscope mirror opening; the second observer sees the reflected image by means of Laurence's plate of glass; and the third places his eye behind the opening in the plane reflecting mirror, and obtains an image from the concave mirror; this third observer is, in fact, almost in the position of a conjugate focus.

Shortly after the establishment of the science and art of ophthalmoscopy Coccius proposed and eventually carried into practice a plan of examination whereby the eye can inspect its own fundus. This consisted in placing close in front of the eye a perforated plane mirror, with its reflecting surface towards the eye, and behind this a flame; the rays passing through the aperture are focussed on the retina; and after emerging from the eye again are made to strike on the mirror close to the perforation, where an image of the illumined portion can be seen; by this means the optic nerve entrance may be seen by the macula lutea of the same eye. Various ingenious *autophthalmoscopes* were subsequently devised; one of the best, though large and complicated, was that of Dr. Heymann, of Dresden. Dr. Giraud-Teulon also constructed one, the principles of which may be learned from the accompanying diagram, copied from the more elaborate figure in his *Précis de la Réfraction et de l'Accommodation de l'Œil*, &c., contained in the supplement to the admirable French translation and annotation of Mackenzie. In this figure the eye *A* is examining its fellow *B*; *F* is the flame, and *M* a concave perforated mirror, held obliquely before the eye, so that rays from *F* shall be reflected on to *c*, a plane mirror which is inclined

at such an angle that the rays falling on it are reflected towards *d*, a second plane mirror, which will reflect them into the eye B; a double convex lens of short focus, *L*, is interposed between

Fig. 13.



B and *d*, by means of which the usual inverted aerial image is formed, which will be seen by A apparently situate beyond the mirror *c*, although absolutely situate between the two mirrors, near *d*; the erect image may be produced by dispensing with the lens *L* and approximating the eyes to the mirrors. By this instrument the whole fundus of one eye may be examined by the other eye. I should mention that it is necessary to dilate the pupil for all these various experiments and examinations.

From the foregoing brief and imperfect sketch it will be seen that the ophthalmoscope is not the result of accident or some happy thought, nor the creation of some one inventive genius, but that it owes its origin to deep research and laborious study, to years of observation, reasoning, induction, and calculation on the part of many, pre-eminent amongst whom will always stand Helmholtz, the founder of ophthalmoscopy. The instrument may possibly be still in its infancy as a means of physical diagnosis, and further modifications and improvements may yet be carried out which will render the ophthalmoscope even more perfect and efficient than it at present undoubtedly is.

ART. IV.—*Notes in Medicine and Surgery.* By PHILIP CRAMPTON SMYLY, M.D., Univ., Dub.; F.R.C.S.I.; L.C.P.I.; Surgeon to the Meath Hospital and County Dublin Infirmary, and to the Institution for Sick Children, Pitt-street.

CASE I.—STRICTURE MANY YEARS STANDING; TREATED BY A MODIFICATION OF MR. HOLT'S DILATOR.

CASE II.—STRICTURE TEN YEARS; LARGE FALSE PASSAGE; THE STRICTURE DILATED BY A MODIFICATION OF MR. HOLT'S INSTRUMENT; NO CATHETER COULD BE SUBSEQUENTLY INTRODUCED; NO BAD SYMPTOMS; NO. 8 INTRODUCED 12 DAYS AFTER.

CASE III.—STRICTURE TWENTY YEARS STANDING; TREATED BY MAISONNEUVE'S METHOD.

CASE IV.—STRICTURE SIX YEARS STANDING; TREATED BY ASPREY'S PLAN.

THE numerous methods which have of late years been brought before the profession for the dilatation of stricture of the male urethra prove that *the method* has not as yet been attained.

The improvements, however, have been great, and the surgeon and the patient have much to be thankful for—the former for simplicity in manipulation and certainty of doing what is intended, and the latter for the alleviation of suffering and diminished risk to life.

The following cases illustrate some of the most recent and improved methods, and also shows that where one fails another may succeed, and that probably, in time, a combination may be attained which, will be *the instrument*, to supersede all others.

CASE I.—Feb. 17th, 1865.—Mr. B. came under observation with an organic stricture of many years standing. About five or six years before he had been treated by the “dilatatory” process, so as to admit a No. 7 or 8. The stricture quickly returned, and he suffered from retention more than once. He had to get up several times during the night, and had great difficulty in even partially relieving his bladder.

The smallest catheter could not be passed into the stricture which was situated about the membranous portion of the urethra.

The worm-like bougie of Maisonneuve's instrument was then tried, and evidently passed through the stricture, but it was found

impossible to get the guide to follow it, though one of my colleagues also tried with much patience for some time. I then passed a No. 3, open at the end, down to the stricture, and made gentle pressure on it, and passed through it the bulbed wire (described by me in a former number of this Journal), and gently pushed it forwards through the stricture. This passed with ease into the bladder, the mouth of the stricture being kept on the stretch by the No. 3. The No. 3 was withdrawn, and a No. 2 forced in, which allowed a few drops of water to escape, thus making sure where the wire was. The No. 2 being withdrawn the dilator is introduced, and the tube passed as in Mr. Holt's operation. No. 10 was then introduced and the urine drawn off. The patient walked home after the operation, and had no constitutional disturbance whatever. He called once a week for three weeks to have No. 10 passed, and then once a month until September, 1865. He did not call again for eleven months. No. 10 passed with ease, no instrument having been passed in the meantime.

CASE II.—R. G., aged forty-three, admitted into hospital on 8th of July with retention of urine. The stricture had existed for ten years, and had been frequently treated by gradual dilatation.

On his admission he was in intense pain, the bladder very much distended, and blood was flowing freely from the orifice of the urethra. A medical man had (according to the patient's account) "passed an instrument up to the end, but could find no water." Mr. Carter, resident pupil, gave him repeated doses of muriated tincture of iron, ten drops every twenty minutes; and finding that a large false passage had been made, sent for the surgeon on duty.

I first tried a No. 10 silver catheter, which passed with ease and with a scarcely perceptible deviation, up to the rings, and a small quantity of thick mucus, with a rather strong smell, mixed with blood, dropped from the orifice. It was withdrawn, and a careful examination was made with a No. 1 gum-elastic catheter, bent shortly at the end, and the opening of the stricture was found in the upper wall of the urethra, exactly above the opening of the false passage—an enormously-enlarged prostate having given the first instrument passed, before his admission, a wrong direction. No entrance, however, could be effected. Twenty-five drops of Battley were given, and a warm bath, and the patient was left for an hour. Several small instruments were tried, but without success. At last a No. 1 wax bougie slipped in.

A wire was then secured to the end of it, and one of Wakley's gum-elastic catheters forced over it into the bladder. The bougie was then withdrawn, the long wire substituted for it, and the dilator being introduced, the stricture was dilated and a quantity of urine allowed to escape before withdrawing the instrument. No. 10 catheter, *secundem artem*, was next tried, but no care or patience could induce it to enter the bladder; every time it went straight into the rectum. The patient was ordered full doses of opium and mulled wine. Shortly afterwards (about half an hour) he got upon his knees, and passed about eight ounces of urine in a good stream.

On the fifth day a small abscess formed in the perineum, which contained pus and a little feces, but no urine; incised.

Twelfth day.—The abscess is almost healed; no urine has ever passed through the wound. No. 8 was passed for the first time.

August 10th.—The patient passes his water in as large a stream as ever, and No. 10 passes with ease. He left hospital in good health, fit for work, and was advised to have an instrument passed once a month.

This case is very instructive, showing that an enormously-enlarged prostate, a large false passage, and retention of urine are no hindrance to the successful employment of Mr. Holt's method. It also shows that it is not necessary to pass a catheter immediately or for many days after the operation.

CASE III.—T. C., aged fifty-eight, admitted into hospital 8th of August, '66, with retention of urine. The stricture was of twenty years' standing, following an injury. He had been several times treated by gradual dilatation.

On his admission Mr. Carter found his bladder very much distended. Tried a large catheter, warm bath, &c.

I found a very tight stricture, into which I could not get any catheter, either gum-elastic or metallic. A No. 1 wax bougie passed with considerable difficulty, and allowed a little very ammoniacal urine to escape after it as it was withdrawn. A No. 10 was then introduced, and pressed against the stricture, some urine following it every time it was withdrawn, giving him great relief. The penis was in such a state of inflammation from treatment previous to his admission that cold applications were ordered for it and a warm poultice to the perineum; and vital dilatation to be continued as long as it gave relief.

For three days vital dilatation relieved him so much—although

the bladder never was emptied; in fact, it was distended so much that it could be felt quite up to the umbilicus—that he would not allow anything more to be done. On the night of the 10th great pain came on and high fever. The penis was still so much inflamed that he would scarcely let it be touched. With great difficulty, and assisted by Mr. Porter, the filiform bougie of Maisonneuve was introduced and followed by the grooved staff. The stricture was then cut, and No. 10 india-rubber catheter introduced, and left in.

There was scarcely any bleeding. The urine (much of which had escaped before the incision) was drawn off through the catheter so as not to act on the wound, which would probably have been much irritated by the highly ammoniacal character of the urine. On the third day the patient could withdraw and re-introduce the catheter himself. He left hospital in three weeks quite well. At the end of October he called at the hospital to report himself. No. 10 passed with ease.

CASE IV.—M. T., aged forty-three, 9th July; retention of urine. The stricture followed a running and swelled testicle six years ago. No catheter could be passed, but a No. $\frac{1}{2}$ bougie was introduced, and left in for an hour, after which the bladder was emptied in a small stream. The wax bougie was introduced several times, and then the seaweed. The latter, however, caused considerable constitutional disturbance. The immediate plan was determined upon; but just at the time the description of Mr. Asprey's instrument appeared in the *Lancet*. It seemed so good I resolved to give it a trial.

“The instrument is composed of a No. 1 catheter, having a little cross-bar which serves as a handle, and can be removed when a screw is loosened. When this handle is withdrawn the dilator, having a conical screw at the end, can be passed over the catheter, and worked by the handle. The dilator may be the size of No. 6, 10, or 12, and has a thin metal cover reaching from the handle to the screw, working smoothly over the tube beneath. This is for the purpose of steadying the penis while the screw works in the stricture; it also prevents any abrasion about the anterior portion of the urethra. The bougie is passed over the catheter into the bladder after the dilator is withdrawn, and will be especially applicable when a small dilator has been used; it may be

left in from twelve to forty-eight hours, according to the wish of the operator. It will be seen from the above description that the principal novelty in the instrument is the use of the conical screw for the purpose of dilatation."

Carefully following these directions, I, with some difficulty, introduced the long catheter. This being overcome, the further steps of the operation were very simple; the screw grasped the stricture and dilated with ease, giving very little pain, and taking but a few seconds to pass through it. The gum-elastic was substituted for the dilator, and the long catheter withdrawn. The patient had no constitutional disturbance, and left hospital some weeks after (having been kept to see whether the dilatation was sufficient), No. 10 passing with ease.

These four cases present some points of interest to the practical surgeon, and tend to teach that while we should not be hasty in laying aside what is good, we should not too exclusively confine our attention entirely to one method of treatment.

As a general rule no instrument that I know of is superior to Mr. Holt's; though in some few, either Maisonneuve's or my modification of Mr. Holt's, may succeed when the former has failed at first.

As to the advantages of the immediate plan, nothing more need be said than to refer to Mr. Holt's statistics, drawn from every part of England and Ireland:—"Thus, out of 742 cases we have eleven deaths, and in not one of these cases is it satisfactorily shown that death was *caused* by the extreme measures that are supposed to be exercised by the adoption of the immediate method."

ART. V.—*Sphygmographic Observations on the Pulse of Typhus.*

By THOMAS WRIGLEY GRIMSHAW, A.B., M.B., Ch. M. (Dub.);
one of the Physicians to Cork-street Hospital; Lecturer on
Materia Medica in Steevens' Hospital.

THE pulse of typhus is too familiar, and its variations too well known to the experienced physician, to require any notice from me as to its value as a means of diagnosis, or the guide it affords with regard to the treatment to be followed, especially in the administration of stimulants.

The object of this paper, however, is to draw attention to the results obtained from an extensive series of observations on the typhus pulse conducted in Cork-street Hospital, by the aid of the sphygmograph of Marey.^a

The physician now daily experiences the benefits derived from the use of instrumental aid in the formation of diagnoses, and in many cases where the information obtained by his unaided senses is quite sufficient for the formation of a correct opinion, these instruments confirm his opinion, and often show results which, although perhaps not of much immediate importance, yet are of considerable scientific value and pathological interest.

It has been with a view of comparing the sphygmograms obtained with Marey's instrument with the sensations conveyed by the pulse to the finger, and of seeing what practical and scientific knowledge may be derived therefrom, that these observations have been undertaken.

The pulse, at the commencement of typhus, is generally found to be quick, without, perhaps, any other change; weakness is then added to the quickness. The pulse is often full and bounding; but a full and bounding pulse is not *always* strong. As to the relation of the pulse to temperature, it will be found that the pulse becomes quicker and weaker, or full and bounding (in certain cases), and of lower tension as the temperature increases; and becomes stronger, less compressible, and frequent, and of higher tension, as the temperature falls. With respect to the eruption of typhus, the variations and state of the pulse have usually a direct connexion with the number and darkness of the spots—*i.e.*, the more numerous and dark the spots, the quicker, weaker, and of lower tension the pulse. Of course there are exceptions to these statements, but as a rule these are the conditions found.

If we look at Figs. 16 and 17, Plate II., we shall see examples of the sphygmogram of health; there may be slight variations from these forms consistent with health, but they are unimportant and easily explained by peculiar conditions of the individual. It will be seen, by taking one of the elevations of the line which represents a single beat of the pulse, that it consists of a nearly vertical up-stroke, and a longer and more oblique down-stroke, interrupted by a small rise, called the dicrotism, or second beat. The up-stroke represents the force of the ventricle in driving the blood into the arteries,

^a Physiologie Médicale de la Circulation du Sang. Paris, 1863.

diminished by the amount of resistance to its onward flow. This resistance depends upon—first, the conditions of the aortic valves, which, if healthy, will not offer much obstruction; second, the resistance in the arteries themselves, depending upon the size of the vessels, the quantity of blood contained in them, and the state of their coats; thirdly, the condition of the capillaries as to whether they are dilated or contracted. One of the chief causes which influence the condition of the capillaries is temperature, and it is of special importance to recollect this when investigating the pulse of fevers. The down-stroke represents the time occupied by the flowing away of the blood through the capillaries and veins before the second stroke of the ventricle takes place. The dicrotism is produced by the rebound of the blood from the obstructions produced by the conditions of the arteries and capillaries. It follows, therefore, that the more elastic the arteries the stronger will be the rebound and the better marked the dicrotism. This elasticity can be destroyed in two ways—either by great flaccidity of the coats of the arteries and capillaries, or by increase of tension in the former, depending upon the rigidity of the arterial walls, the fulness of the arteries, and the resistance of the capillaries. We find both these conditions developed during the course of typhus fever.

The principal changes in the form of the typhus pulse seem to depend upon the variations in arterial tension; this will be best explained by reference to Plate III. Fig. 1 may be taken to represent, diagrammatically, the pulse of typhus if the actual tension were at its lowest—so low, that there appears to be no obstruction to the onward flow of the blood. Here we find that the tension of the arteries is so much diminished before the recurrence of the stroke of the ventricle that it offers no resistance to the refilling of the vessels, and the down line of the sphygmogram falls to its lowest. If, however, the tension is increased, as in Fig. 2, which represents the amount of Fig. 1, between lines *a* and *d*, with the elevation joined by horizontal lines, the arteries having still sufficient tension to obstruct the onward flow of the blood until the occurrence of the next ventricular contraction, the first beat is, as it were, caught up by the second; Fig. 3 represents a pulse of still higher tension. A pulse similar to Fig. 1 is comparatively seldom found in typhus (Fig. 5, Plate I.; and Fig. 6, Plate II.), but the form represented in Fig. 2 may be looked upon as that characteristic of the disease during its height. These explanations are necessary as the

sphygmograph has not yet come into such general use as to be familiar to the profession at large.

I have already pointed out that the characteristic pulse of typhus is (as shown by the sphygmograph) that of very low tension, with arteries reduced to the lowest state of flaccidity—and that is just what we might expect from the great weakness and high temperature of the patient during the disease. The weakness of the muscular system, as shown by the patient's loss of power, and the diminution of the first sound of the heart, of course, affects the fibres of the muscular coat of the arteries in the same way as those in other situations, thus diminishing the arterial tension. Again, the high temperature is accompanied by an increase in the calibre of the capillaries, thus also diminishing the arterial tension. In Marey's book, although a great number of pulse writings are delineated, we do not find any showing such a state of flaccidity and utter loss of arterial tension as found in the pulse of typhus. The nearest approach to this condition which Marey has delineated, is that found in puerperal fever.^a An approach to the typhus form is also delineated by him for erysipelas (erysipele grave), and for pyemia (infection purulente) after amputation. The resemblance between these diseases and typhus as to their adynamic conditions is well known.

In fever practice the physician may usually divide his cases into two classes, the one following a short, the other a long course to recovery. The former are the less, the latter the more severe. In making thermometric observations on typhus, it will be found that the cases may be divided into two classes: those corresponding with the cases following the short course, which attain their maximum temperature on the sixth and seventh day—and those which follow the long course, which attain their maximum on the eighth or ninth day. My attention was first directed to these two classes of cases by Dr. J. Southey Warter, in a private communication. He has since made his observations on this point public in the St. Bartholomew's Hospital Reports.^b I have examined all my notes of temperature, and found that they corresponded to a great extent with Dr. Warter's observations, though differing in some respects; he finds the seventh and ninth days are the periods of maximum temperature in the classes of cases I have just mentioned; whereas I should

^a Marey—*Circulation du Sang*, p. 390, Figs. 102, 103, and 104.

^b Remarks on the use of the Thermometer in Acute Disease. St. Bartholomew's Hospital Reports, Vol. ii. London, 1866.

rather say the sixth and seventh for the one, and the ninth and tenth days for the other class. In fact, I find the seventh and tenth days are the common days of maxima.^a Dr. Warter also considers that the ninth day is the commonest for maximum temperature in his cases; now, in mine, I find the maximum is usually attained before the ninth day. This is what might have been expected, as I have remarked elsewhere,^b that typhus seems to run a more severe course in London than in Dublin; and as the maximum temperature occurring late in the disease corresponds (in my experience, at least) with the more severe cases, we might expect that this difference in results between Dr. Warter's observations and my own would appear, his observations having been all taken in London, and mine in Dublin.

I may also remark, when upon this interesting subject, that when typhus was less fatal in the commencement of last year (1866), the maximum temperature of the cases was attained sooner than when more fatal during the Autumn months. I make these remarks upon the temperature here, because I have found that the variations in the pulse correspond, in the majority of cases, with those in temperature, the higher the temperature, *cæteris paribus*, the lower the tension, and the less marked the dirotism of the pulse, and *vice versâ*, as before stated.

The pulse characteristic of typhus is, as a rule, best marked in proportion to the darkness of the spots; as the spots appear the pulse gradually loses its natural form, showing a low tension and want of dirotism; as the spots vanish the tension increases and the dirotism returns; but after its return, and as the patient approaches convalescence, the dirotism, in most cases I have observed, again disappears, but from a different cause, which seems to be a contracted and pinched state of the muscular fibres of the arteries, similar to that of the other muscles, which is only too evident, in the appearance of fever convalescents. In Plate I. are delineated the various forms of pulse found in typhus cases running the short course to recovery. Fig. 1 represents the pulse of the

^a While the author was writing the above Dr. Warter was lying ill of typhus, which disease he had contracted while pursuing further his thermometric observations. In a few days he passed from this world, a martyr to medical science. His loss will be mourned by all who take an interest in the application of exact science to practical medicine.

^b Temperature of the Body in Fever. Medical Press and Circular, Feb. 21st, 1866.

second day of the disease (of course observations on typhus so early in the disease are not easily obtained). I have only been able to observe two, the result being the form given in Fig. 1, which does not deviate much, except in frequency, from a natural pulse. Fig. 2 represents the state on the third day, showing a greater deviation from the natural form. Fig. 3, the fourth day; Fig. 4, the fifth day; Figs. 5, 6, 7, and 8, represent various forms found on the sixth day, the more common being Figs. 6 and 8; the forms 5 and 7 being found in the more severe cases. Figs. 9, 10, and 11, represent the pulse of the 7th day; Fig. 9 being the most common form. The similarity of the forms on the sixth and seventh days will at once be remarked, as also that of the eighth day, Fig. 12. On the ninth day we usually notice a return of the dicrotism (Fig. 13), as the temperature falls, and the spots begin to disappear, but the low tension still remains; the tension now begins to rise, as seen in Fig. 14, tenth day; Fig. 15, eleventh day; and Fig. 16, twelfth day; the amplitude of the pulse tracing is not always so great on the eleventh day as seen in Fig. 15. In Fig. 17, which corresponds with the thirteenth day, we see the dicrotism again becoming indistinct until the fourteenth day. In Figs. 18 and 19 we find it completely gone; from this time the dicrotism again appears, and the pulse returns gradually to its natural form, as seen in Figs. 20, 21, and 22 (Fig. 22, which is marked in the Plate for day seventeen, does not, I think, exactly represent what probably will be found on further observations to be the average tracing for that day; I think it should be more like Fig. 21). The representations in the Plate are, as nearly as possible, fac-similes of tracings selected as being most typical of the days to which they are stated to correspond. I consider this a preferable mode of drawing up a series of daily sphygmograms to constructing average diagrams for each day. The vertical lines in the Plate are drawn at intervals corresponding with half a second, and are equal to the half-second divisions of my transparent sphygmographic scale.*

In Plate III., Figs. 1 to 15 represent a series of sphygmograms taken from a case of typhus belonging to the classes which run the long course, having their maximum temperature on the ninth or tenth day. Fig. 1 corresponds with day eight of the disease; Figs. 2, 3, 4, &c., up to 15, with days 9, 10, 11, &c., up to day 22 respectively. It will be observed that the character of low tension

* Med. Press and Circular, Aug. 22nd, 1866.

and absence of dicrotism is present in this series for a much greater number of days than in that corresponding with the short course cases represented in Plate I. At Fig. 4, a peculiar form of pulse occurs, which I have usually found accompanying head symptoms, chiefly violent headache, which is generally relieved by leeches to the temples, and cold applications. In Fig. 20, Plate II., a specimen is given of a sphygmogram, also accompanying head symptoms, which occurred in a large, heavy, plethoric man, with full bounding pulse, who was in a very peculiar and nearly comatose condition; this case was relieved by the application of leeches to the temples. We sometimes meet with pulses characterised by low tension and but slightly marked dicrotism, in smart cases of simple continued fever, as in Figs. 18 and 19, Plate III. In cases of typhus with *subsultus tendinum*, it is impossible to get a clear sphygmogram; sometimes only a slight effect of the beat of the artery can be discovered, as shown in Fig. 22, Plate II.; or it is quite obscured by the action of the tendons, as shown in Fig. 21, Plate II. In cases of well-marked typhus, without spots, we shall sometimes find that the sphygmogram never loses its dicrotism during the whole course of the disease. I have not yet observed a similar occurrence in a maculated case. Does this resemblance between the pulse of unmaculated typhus and febricula (Figs. 18 and 19, Plate II.) tend to prove any closer relation than is usually supposed to exist between those two febrile affections? The pulse delineated in Plate III., Fig. 4, was very peculiar, and existed for one day in a case of unmaculated typhus. I am quite unable to offer an explanation of this form. I could not detect any cardiac lesion.

I have not yet had much opportunity of observing the pulse of typhoid fever; but in the few cases I have observed in this disease, the most common pulse tracing is that figured by Marey,^a the dicrotism being usually well marked. In a mixed case of typhus and typhoid I observed the forms figured in Plate III., Figs. 5, 6, and 7. Fig. 7 shows a near approach to the typhus form.

What practical use can we make of the information offered us by the sphygmograph in typhus fever? At present not *very* much, excepting the advantage we derive from a more accurate and definite knowledge of the general symptoms of the disease. As a guide in treatment, it will be found that the pulse with the lowest tension, and least developed or absent dicrotism, is the one

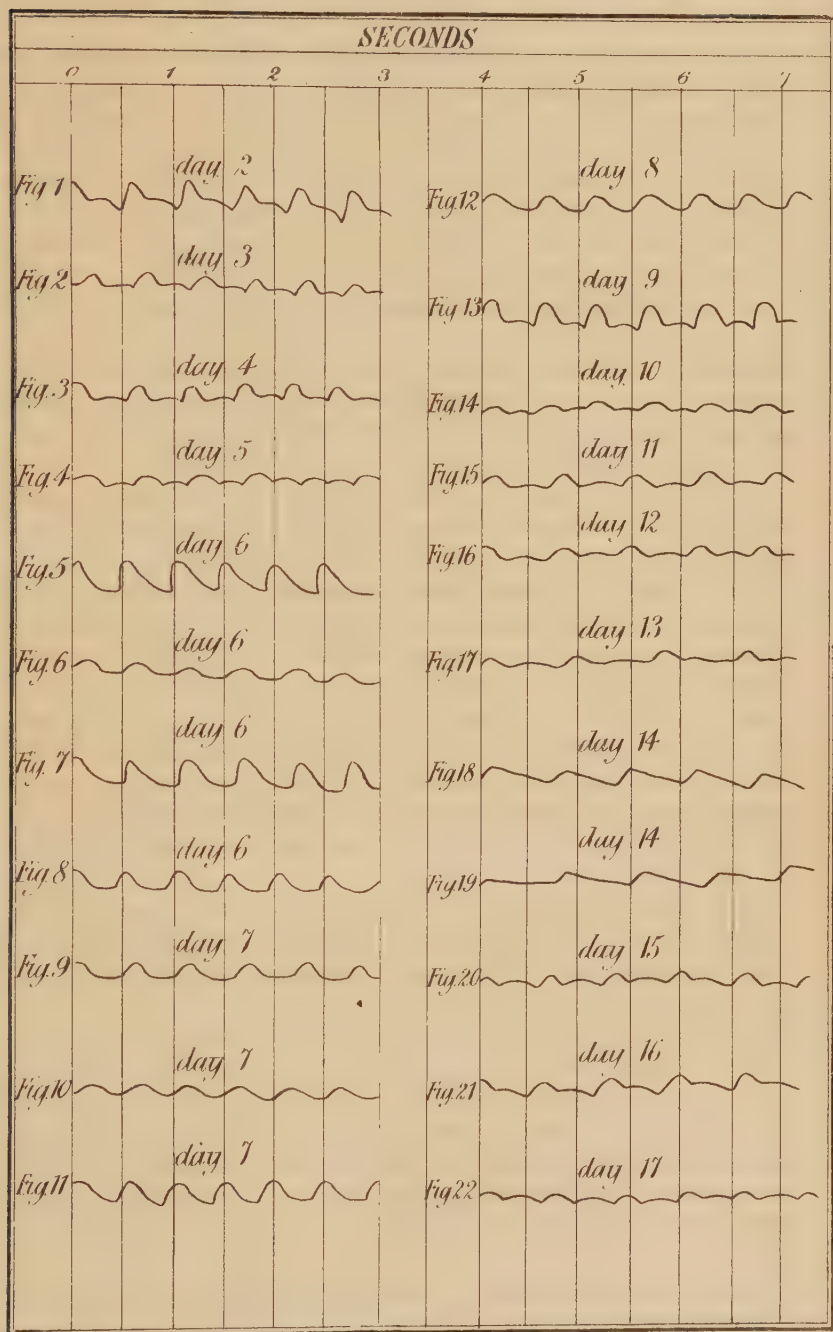
^a Marey—p. 389, Fig. 100.

indicating the largest requirements of stimulants. This, however, is only expressing, in other words, the fact so much relied upon, that the character of the pulse is the guide in the use of stimulants. The character of the pulse of the patient will, in the majority of cases, be as easily estimated by the *tactus eruditus* of the experienced physician as by the sphygmograph. To the inexperienced, however, the sphygmograph will prove a valuable guide in treatment, as its written indications, as observed by others, will constitute a positive standard of comparison, which described sensations cannot possibly do. Perhaps, however the most important practical point is, that the sphygmograph appears to give us more accurate perceptions of the condition of the heart than the finger does. Dr. Stokes has pointed out^a that the sensations conveyed to the finger, when applied over an artery, do not always indicate the exact condition of the heart, as determined by the stethoscope applied over the chest. From my experience, the sphygmograph gives this information more accurately than the finger.

With regard to prognosis, there is no doubt the gravity of the case is measured by the character of the pulse. I have found that the sphygmograph shows an improvement in the pulse by demonstrating a slight return of its dirotism, before any sensible improvement could be ascertained by the finger. In one very protracted and severe case particularly, I was able, on the twelfth day, to predict an improvement in the state of the patient from the improved sphygmogram, though all the other symptoms remained the same, and the pulse to the finger presented no appreciable alteration.

The observations upon which my remarks are founded were nearly all taken in Cork-street Hospital, the majority of the patients observed being under my own care, but many of them under that of my colleagues, Drs. Kennedy and Mason, to whom I have to return my best thanks for the facilities they have always afforded me in the pursuit of scientific enquiry. I followed about sixty cases throughout the course of their disease, the observations upon which were checked by many casual ones upon other cases. At the time the sphygmograms were taken, notes were also taken of the sensations conveyed by the pulse to the finger; the temperature in the axilla and rate of respiration were recorded as well as the other

^a Stokes on Disease of the Heart. 1854. P. 134; and Dublin Journal of Medical Science, 1839, p. 15.



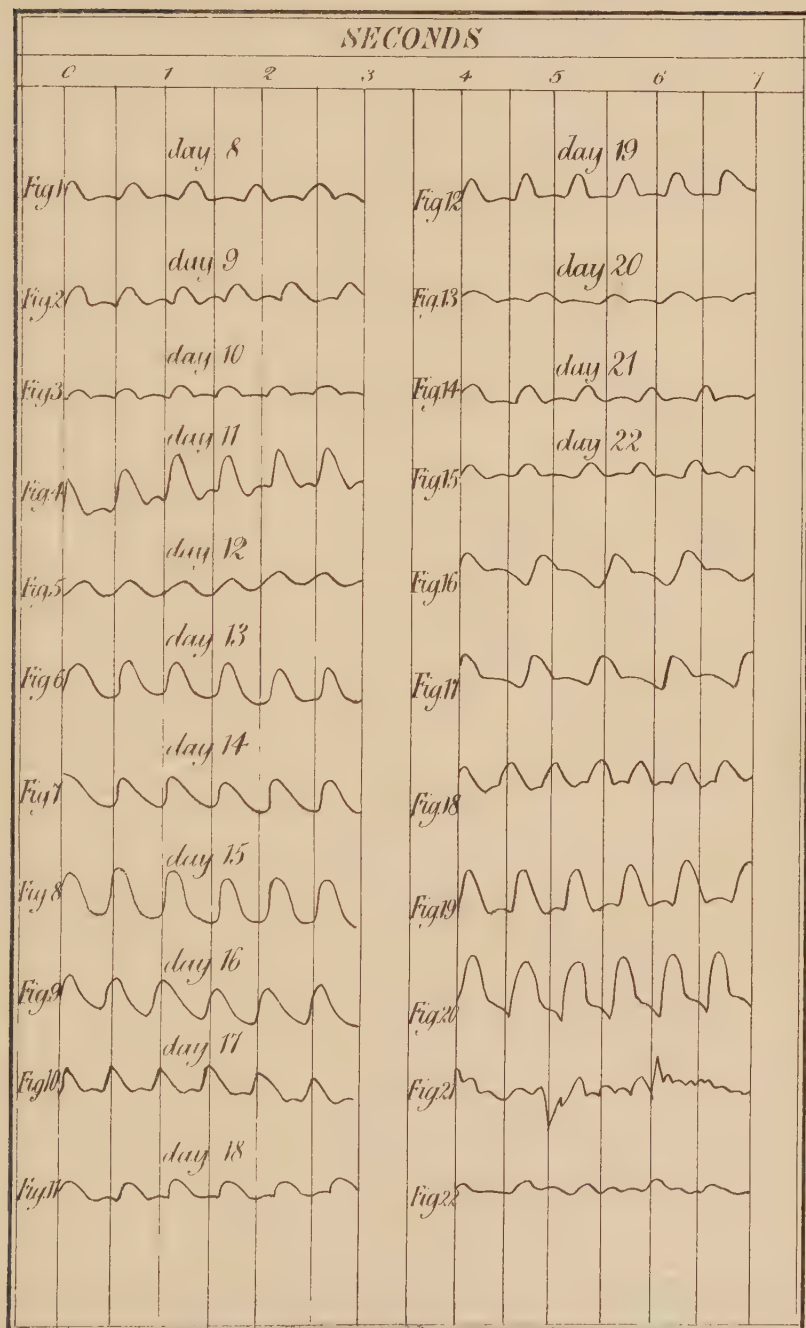




Fig 1

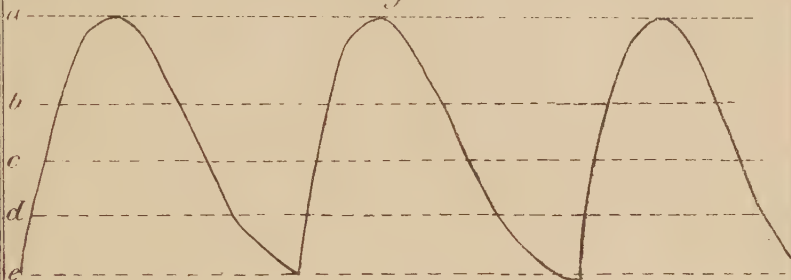


Fig 2

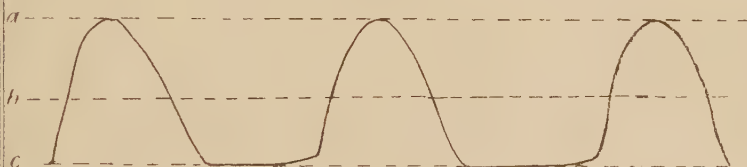


Fig.3

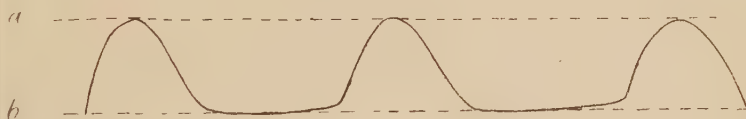


Fig 4



Fig. 5



Fig 6



Fig. 7





conditions of the patient. Altogether, about 3,000 observations of pulse, respiration, and temperature, have been made by me, which I think must be considered sufficient foundation for the foregoing remarks. I hope they will be of interest to the profession, and eventually add, in some degree at least, to our knowledge of the symptomatology and treatment of typhus fever.

Explanation of Plate I. Pulse of Typhus running a Short Course to Recovery.

Fig. 1.—Pulse of 2nd day of Typhus.

Fig. 2. " 3rd " "

Fig. 3. " 4th " "

Fig. 4. " 5th " "

Figs. 5, 6, 7 and 8.—Pulses of 7th day of Typhus.

Figs. 9, 10, 11, and 12.—Pulses of 8th day of typhus.

Fig. 13.—Pulse of 9th day of typhus.

Fig. 14. " 10th " "

Fig. 15. " 11th " "

Fig. 16. " 12th " "

Fig. 17. " 13th " "

Figs. 18 and 19.—Pulse of 14th day of Typhus.

Fig. 20.—Pulse of 15th day of Typhus.

Fig. 21. " 16th " "

Fig. 22.—Pulse of 17th day of Typhus, running the short course, but not an average specimen; the tracing should be more like Fig. 21.

Explanation of Plate II.

Figs. 1 to 15.—Pulse illustrative of a case running the long course to recovery, the record commencing on the 8th day.

Fig. 16.—Healthy female pulse.

Fig. 17.—Healthy male pulse.

Figs. 18 and 19.—Pulses of severe simple fever, at the height of the disease.

Fig. 20.—Pulse found often to accompany severe headache, easily relieved by leeching.

Fig. 21.—Tracing produced during an attempt to take a sphygmogram from a patient with severe *subsultus tendinum*.

Fig. 22.—Tracing produced by the combined action of the pulse, and moderate *subsultus tendinum*.

Explanation of Plate III.

Figs. 1, 2, and 3.—Diagrams explanatory of the variations in tension of the typhus pulse.

Fig. 4.—A remarkable form of pulse found in one case of typhus for one day.

Figs. 5, 6, and 7.—Forms of pulse found in cases of mixed typhus and typhoid fever.

ART. VI.—*On Polypus of the Uterus; with Description of a Modification of the Ecraseur for the Removal of Intra-uterine Growths.* By LOMBE ATTHILL, M.D., Dubl.; Fellow of the King and Queen's College of Physicians in Ireland; Examiner in Midwifery, Queen's University,[†] and ex-Assistant Physician, Rotundo Hospital.

DR. MARION SIMS, in a paper published in *The Lancet* in Nov., 1864, remarking, with great propriety, on the advantages arising from the use of the sponge tent in cases of intra-uterine disease, says:—"That before their introduction, if we suspected, from the rational symptoms, the existence of an intra-uterine polypus, we could only wait from month to month—sometimes from year to year—for it to grow and force its way into the vagina, before we could interfere surgically for its removal; but now we no longer let our patients bleed till they become bloodless and dropsical, but we ferret out at once the source of the mischief and remove it from its once secure hiding place." "This," he adds, "is a great advance in uterine surgery, and no man of twenty or thirty years' experience can look back on the days of Ergot and Gouch's canulæ, and contrast them with the present time of sponge tents and the ecraseur, without a thrill of delight at the progress of our noble calling." I quote the foregoing passage because it is a graphic description of the change which has taken place in the treatment of uterine disease; for, even in my own experience, though falling short of even the twenty years, I have seen a patient die from the effects of the hemorrhage caused by an intra-uterine polypus, which we knew existed but could not reach. I think, however, that Dr. Sims over-estimates the value of the ecraseur, or, perhaps, more correctly, under-estimates the difficulty of its application in some of these cases. The following brief details of the history of a patient who was placed under my care during the past Summer, by my friend Dr. Head, will, I think, prove interesting, as offering an example of a life saved by the use of the sponge tent and ligature, while, at the same time, it tends to illustrate the difficulty of adjusting the chain of the ecraseur round an intra-uterine polypus.

She was an unmarried woman, aged about forty-five years, and when I saw her was very weak, greatly emaciated, and presenting a remarkably unhealthy appearance; so much so as to impress the observer with the idea that she must be labouring under malignant

disease. She suffered incessantly from sickness of the stomach, and for weeks past had never been free from hemorrhage. She stated that formerly she had been a very healthy woman, but that some eighteen months ago she began to perceive the catamenia, which hitherto had been in all respects natural, to become much more profuse; then that they returned at short and irregular intervals; and finally, as already stated, that she was never free from a sanguineous discharge. During the whole of this lengthened period she suffered but little pain, though at intervals severe paroxysms came on, caused evidently by uterine contractions, for they always terminated in the expulsion of clots, after which she experienced relief. She resided in the country, and there had tried various remedies, mainly, I believe, tonics and astringents, which had been prescribed for her relief; but as they failed to check the hemorrhage, she came to town for further advice.

On making a vaginal examination I was surprised to find that the uterus was free from malignant disease. It was low in the pelvis, greatly but evenly enlarged, globular in shape, and very firm to the touch; the cervix was completely obliterated, and the os closed, admitting with difficulty the point of a uterine sound, which, however, once introduced, passed freely into the cavity of the uterus to the depth of nearly five inches; the existence of an intra-uterine tumour was beyond doubt, but whether it was a fibrous tumour or polypus had yet to be ascertained. I believed it would prove to be a polypus, basing this opinion on the fact that the uterus was so evenly enlarged, and also on the absence of pain, which, as far as my experience goes, is a nearly universal accompaniment of fibrous tumours of the uterus. As the patient was in a very debilitated state, and losing blood rapidly, it was absolutely necessary to act promptly. I therefore proceeded without delay to dilate the os, and, after a little difficulty, succeeded in introducing into it a small sponge tent; this, on its removal, was found to have opened the os, so as to admit the tip of the finger; a larger tent was at once introduced, followed, on its removal, by one still larger, which completed the dilatation of the os to the size of a crown piece; this process occupied two days. The finger could now be introduced freely into the uterus and swept round the tumour, the attachment of which was very high up and to the right side. The diagnosis of polypus being thus verified, its immediate removal was decided on. Dr. Beatty and Dr. Kidd, who saw the case with me, assisting in operation. At the suggestion

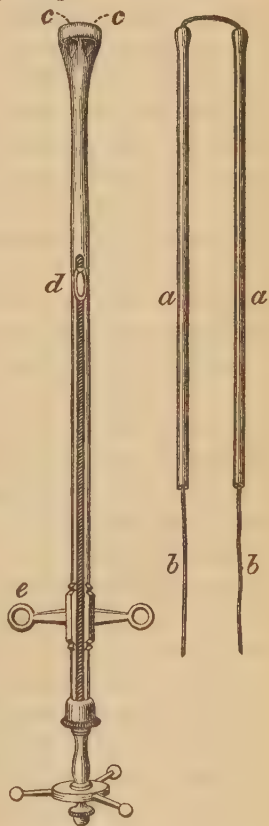
of Dr. Kidd, who kindly lent me the instrument, I determined to try Dr. Marion Sims' *porte chaine ecraseur*. The first step in the operation was to endeavour to draw the polypus partially through the dilated os; for this purpose a double hook was fixed in it, but the pedicle was so thick and dense that it was quite impossible to accomplish this; the hook, however, as it served to steady the uterus, was not removed till after the operation was completed; the end of the *ecraseur* was now introduced into the uterus, and I succeeded, without difficulty, in carrying the chain to the fundus, and consequently beyond the attachment of the polypus; but the moment I began to expand the spring blades the difficulty commenced, for the uterus, probably from its never having been impregnated, was most unyielding; and though I twice succeeded in expanding the blades, the chain on each occasion became displaced, and slipped off the spring blades, and I consequently failed to secure the polypus. I now made an effort to use an ordinary wire *ecraseur*, carrying the wire up by means of a gum-elastic catheter; this plan, however, failed also, and as it was evident that the patient would speedily sink from the effect of the hemorrhage unless we could remove the polypus, I had recourse to the *canulæ* of Gouch, and with them, though not without considerable difficulty, succeeded in passing a strong ligature of double whip-cord round the pedicle; once, however, applied it never slipped; and having been moderately tightened, was secured; the patient was greatly exhausted after the operation; some vomiting followed, which was checked by ice, and a tolerable night's rest was secured by an opiate enema. On the following and succeeding days the ligature was tightened, morning and evening; this was most satisfactorily effected by means of a small winch, which Dr. Beatty has most ingeniously adapted to the end of the connecting rod of the *canulæ*, and which, when sufficiently tightened, is secured by a screw; this worked admirably, and saved much trouble.

Five days elapsed before the ligature cut through the pedicle, and even then the polypus remained in utero. I succeeded, however, in extracting it with a pair of small forceps; it was then about the size of a large egg, but originally must have been much greater; in structure it proved to be purely fibrous.

The woman recovered slowly, but after the lapse of three weeks was able to return home convalescent.

This case, though presenting nothing very unusual, is instructive. We may consider the result eminently satisfactory; for, without

doubt, this woman would have died had the removal of the polypus been delayed but a few days longer: but the means by which its removal was accomplished were not what we would desire; for though the dilatation of the os was rapidly effected, my failure in applying, first, Dr. Marion Sims' ecraseur, and subsequently an ordinary wire one, compelled me to have recourse to the old and objectionable method of the ligature, an operation only justified by the urgency of the case—an operation always disagreeable, and often followed by dangerous and even fatal consequences. In the present instance not only was the inseparable accompaniment of a very fetid discharge present, but I greatly dreaded the occurrence of blood-poisoning or some low form of peritonitis, as the patient had more than one well-marked rigor, and vomiting returned almost daily. I therefore bitterly regretted my failure in applying Dr. Marion Sims' instrument; perhaps the failure was due to my own want of skill, but certainly I did not give up the attempt till I was perfectly satisfied that snare the polypus with that instrument, I could not. The instrument, though a very ingenious one, is complicated, and in a case like the present, where the vagina was very narrow and the uterus extremely rigid and completely filled by a very firm tumour, its application became simply impossible. These circumstances led me to consider whether a less complicated instrument could not be devised, which would unite to the great advantages attending the rapid action of the ecraseur the facility of applying the ligature afforded by the canulæ of Gouch: and it occurred to me that if the extremity of the ecraseur were modified, so as to allow the ordinary canula of Gouch to pass through it, we would have a simple and efficient instrument. I have had an ecraseur, so modified, made by Weiss, and append a woodcut to exemplify its application. With the canulæ *a a*, a wire rope, *b b*, of considerable strength, can be passed round the pedicle



of the polypus; the canulæ are then to be passed through the opening *cc* in the extremity of the ecraseur: the ecraseur is then to be pushed up, guided by the canulæ, till it comes in contact with the pedicle of the polypus, filling the place exactly that the connecting rod of Gouch did in his instrument; the canulæ can then be entirely withdrawn, and the wire rope, being attached to the ecraseur at *e* and *d*, the operation is completed, as if we were using an ordinary wire ecraseur. If the engraving given here be compared with that in Dr. Gouch's work on *Diseases of Women*, it will at once appear that it differs from his instrument only in the substitution of an ecraseur for the connecting rod, but a great advantage is obtained in the power conferred by the ecraseur of enabling us to complete the operation at once, and thereby save the patient from the great dangers often following the application of the ligature in the old way: and I think if this simple modification of the ecraseur enables us to succeed in even but a moderate proportion of cases it will be no slight gain, and will prove a useful addition to our obstetric instruments. I am well aware that cases will be met with which will resist the action of the wire rope, or break it; but they are rare; and if a strong twisted wire rope be used where the pedicle seems likely to offer much resistance to the action of the instrument, they will prove still more so. In conclusion, I wish to add that I do not desire to depreciate the merits of Dr. Marion Sims' ingenious instrument; but it is too complicated, and I feel that it is inapplicable to many cases. Dr. Braxton Hicks' instrument, too, though possessing the advantage of simplicity, will often fail in carrying the wire rope round the pedicle if seated at a distance from the os, and will always be difficult of application. I think that the modification I have suggested, which in reality is the adaptation of the canulæ of Gouch to the ecraseur, will be found superior, at least in facility of application, to any yet made.

The following case illustrates how little inconvenience may, under certain circumstances, be caused by even large polypii—how slight may be the symptoms, and how nature sometimes effects a cure:—

I was requested by a nursetender to visit a woman, the wife of a cabman, who, she informed me, had been confined under her care three days previously, after a natural labour, of a strong well-nourished child. I found her to be a healthy robust woman, aged about thirty. She stated that when in the ninth week of this her

third pregnancy, having exerted herself to raise a heavy weight, she "felt something to give way inside" her, and immediately after perceived a tumour to protrude through the vulva; smart hemorrhage followed, but this soon ceased, and the tumour receded.

During the remainder of her pregnancy she continued to enjoy good health, and experienced no inconvenience unless she were to take a long walk, or make any unusual exertion, in either of which cases the tumour would again partially protrude, but it receded again on her lying down; there was also, on one or two occasions, slight hemorrhage. The midwife who attended her in her confinement, which appeared to have been rapid and easy, did not perceive anything unusual till, on the third day after delivery, she found a large mass protruding through the vulva—when, being alarmed, she came for me. I found, just as she described, a soft mass, of considerable size, lying in and nearly filling the vagina. At first I thought that was a portion of the placenta which had been left behind, it resembled it so much in appearance, but a careful examination proved it to be a polypus, which could be easily traced up to its attachment by a long and slender pedicle to the posterior lip of the os uteri; some hemorrhage followed the examination. As the woman was so recently delivered I thought it better to defer for a short time the removal of this polypus.

On visiting her again, after the lapse of two days, I found the pedicle shrivelled up, and the polypus itself beginning to decompose. I, therefore, divided the pedicle with a pair of scissors, at a distance of about half an inch from the os, and removed the mass, which proved to be a soft mucous polypus. The woman recovered without a bad symptom.

ART. VII.—*A Comparison between the Accidents which have Occurred in Scutch Mills, and in Factories Subject to Government Inspection, as they have come under my Notice during the Eight Years of my Connexion with the County Down Infirmary.* By JOHN K. MACONCHY, M.B., F.R.C.S.I.

IN the November number of the *Dublin Quarterly Journal* Dr. Babington, of the Londonderry Infirmary, has given some valuable information on the subject of the frequency of accidents occurring in scutch mills. My present object is to give further evidence on

this matter, and, if possible, aid in drawing attention to it, as I have no doubt there would be a diminution in the number and severity of accidents in these mills if they were subject, as far as practicable, to the same rules and inspection as factories.

Many of the accidents which have come under my notice have arisen from circumstances which improved machinery and due caution in management would obviate.

The worst source of injury and loss of life has been the use of the old wooden rollers, through which the flax has to pass several times; they are served from a narrow table, to which the flax is returned after passing through, and from which it is again started, so that the hands of the operative serving them are in frequent and close proximity to danger. The patent rollers used in most new mills are much smaller, capable of being completely covered, served from a table of over three feet long, and the flax has only to pass through once, and is delivered at the opposite end to its entrance, so that the operative serving them need never be in danger; and, owing to the small size and the fencing, even if he were caught, could not be so seriously injured.

Other sources of accident, such as carelessness in not giving due notice when the machinery is about being put in motion, want of proper fencing, and imperfection in the machinery, would come properly under the cognizance of a Government Inspector.

Appropos of imperfect machinery. I lately visited an old mill in which the handle for putting the rollers out of gear was held to a beam of wood by a holdfast; the removal of the holdfast occupied at least two minutes; and when removed the handle itself would not consent to stir; had a finger been caught in these rollers the arm would have been drawn in to the shoulders before they could have been stopped.

It often happens in these mills that the owners of flax, or their servants, help in the rolling of it, and not being accustomed to machinery, they are peculiarly liable to accident. Any means of preventing this practice would be most desirable.

In order to estimate the comparative frequency of accidents arising from the two sources mentioned in the heading of this paper, I have endeavoured to arrive at some approximation to the numbers employed in each, and for how much of the year they are so employed in this district. This infirmary receives all the accidents of consequence occurring in four factories, which employ from 2,700 to 3,000 operatives constantly. In the same district there are now,

as far as I can ascertain, fifty-nine scutch mills, but at the beginning of the eight years of which I treat there were hardly thirty; at present the mills could employ about 2,500, but so many as this, are never employed even in the busiest part of the scutching season, which lasts three or four months, the work then gradually decreases, and for five or six months most of the mills are idle, so that I am sure I over-estimate the numbers employed in scutch mills on the average of the whole year at a quarter of those employed in the four factories. Were accidents in proportion to employment there would be at least four from the factories for one from the mills. How different the real proportion is will be seen at once by the table subjoined:—

	Factories.	Mills.
Fatal injuries,	0	6
Injuries involving loss of limb,	2	7
Injuries involving serious and permanent deterioration of a limb,	3	3

Even this table does not show fully the disproportion, for reference to the list of accidents will show a disproportion in severity which cannot be sufficiently expressed in a tabular form.

During the eight years the factories have occasioned the loss of two forearms and seriously impaired three arms, whereas, in the same time, the scutch mills have killed six people, seven survive minus a limb, and others with seriously impaired arms. Twelve of the sixteen mill accidents are more severe than any of the five factory accidents. The following accidents are all on the books of the infirmary, except two, in which the injuries were too quickly fatal for the removal of the patient.

Factory Accidents.

- | | | |
|---|---|--------------------------|
| 1. Mary Maguiness, | } Amputation of forearm, | } From revolving wheels. |
| 2. Matthew Campbell, | | |
| 3. John Bell, partial amputation of hand, | | |
| 4. Charles Walsh, | } One arm seriously and permanently deteriorated, | } Caught in the hackles. |
| 5. Edward M'Cartan, | | |

Scutch Mill Accidents.

1. Both arms caught in the rollers, and chest crushed against them. This man died almost immediately.

2. The left arm caught in rollers; ribs of that side broken; died the day after the accident.

3. Robert Smith, aged fifteen, admitted Jan. 19, 1859. Left arm drawn in by rollers; soft parts lacerated; blood-vessels and nerves torn; bones comminuted; at least two ribs, left side, broken; amputation at the shoulder joint; in a few hours the side became emphysematous where the ribs were broken. Died the day after admission.

4. Richard Hanlon, aged thirty, admitted 30th September, 1863. Right arm drawn in up to shoulders by rollers; no bone broken, only a slight wound in the upper and inner part of the arm; brachial artery not pulsating; arm evidently infiltrated with blood; cold; sensation destroyed. He positively refused to submit to operation till the next day; acute mortification set in during the night, extending to the wall of the chest. Died 4th of October.

5. James Bothwell, aged sixteen, admitted 2nd November, 1863. Right arm drawn in by rollers; humerus broken close below articular head; axillary artery and plexus of nerves torn; thoracic side of axilla bruised and torn; amputation at shoulder joint, but a clean wound could not be left; wound became sloughy; third day secondary hemorrhage, which was arrested, but he never recovered this second loss of blood, and died on 13th November, 1863. Were I called on to treat a similar case again, viz., amputation at the shoulder necessary, and some sloughing in the wound inevitable, my first step would be ligature of the third stage of the subclavian artery, trusting to the branches of the thyroid axis for the vitality of the wound and flap, and having the ligatured part of the artery as far as possible from the probable sloughing.

6. James Ringland, aged seventeen, admitted 20th January, 1865. Was cleaning behind handles when the machinery was put in motion; one leg and thigh torn, and bones broken in many places; the other less injured; body crushed; never recovered from collapse. Died 21st January, 1865.

7. Joseph Cleland, aged nineteen, admitted 1st December, 1863. Hand and wrist injured by wheels; amputation of forearm. Recovery.

8. Eliza Harbison, aged fifty, admitted 30th September, 1863. Left arm drawn in by rollers to above elbow; would not consent to operation; more than six months after forearm dropped off.

9. Isabella Coulter, aged sixteen, admitted 23rd December, 1863. Not a regular mill operative, but helped in the rolling; caught by

rollers; right arm drawn in to near shoulder; amputation at shoulder-joint. Rapid recovery.

10 James Fisher, aged twenty-nine, admitted 2nd November, 1864. Both arms drawn in by rollers; right arm amputated above elbow; left saved; now a tolerably useful limb; has recently been a patient of the infirmary, with lumbar abscess.

11. Edward Rogan, aged thirty, admitted 3rd February, 1865. Left arm drawn in by rollers; muscles, vessels, nerves, and bones all injured; amputation at shoulder-joint. Recovery.

In this case, it being the left arm, I operated by dissecting up the flap, and think it a more tedious proceeding, more troublesome in securing the arteries that surround the neck of the humerus, and certainly clumsy-looking surgery.

I would prefer to make the flap standing at the outside of the patient's arm, transfixing is so much easier, and more quickly performed.

12. John Hinds, aged forty-three, admitted 22nd March, 1865. This was a most peculiar accident. The right arm was drawn in by the rollers to near the shoulder; *no bone was broken, nor was there the slightest solution of continuity of the skin*, but sensation was destroyed; no pulsation to be felt in any artery below the axilla; the limb was cold, and slightly discoloured.

Every effort was made to try and convince the man of the danger and uselessness of trying to keep the limb; but when he saw no wound, and knew no bone was broken, he would not be convinced. So, exactly one year after, the forearm bones came off. He has a humerus covered with discoloured skin, devoid of sensation, and still largely ulcerated. I saw him a few days ago, so can answer for the state a year and nine months after the accident.

13. Pat M'Conville, aged sixteen, admitted 17th October, 1865. Caught in the metal wheels which move rollers; right arm injured to above elbow; artery torn; amputation in the upper third of arm. Recovery.

14. John Armstrong, aged fifty-four, admitted 22nd May, 1861. Caught in rollers; comminuted fractures of arm and forearm. Recovered, with a permanently damaged limb.

15 Pat. Gribben, aged thirty-four, admitted 31st October, 1862. A farm labourer, not a regular mill operative. Caught in rollers; vessels and nerves escaped; limb saved, but permanently deteriorated.

16. Thomas Martin, aged twenty-nine, admitted 18th November, 1865. Back of the hand struck by handles; extensor tendons destroyed; a permanently deteriorated limb.

There have been many injuries of less severity than the foregoing from both sources, and there is not the same disproportion between them that there is in the severe accidents; this I attribute to the want of fencing in the scutch mills. When machinery is well fenced, a finger or two may be caught, and the tops injured or lost, but the whole arm cannot be drawn in, and the more fatal forms of accident are avoided.

I believe reasons exist why the present Factory Act could not well be extended to scutch mills, at least without some modifications; yet if, in other localities where flax is much grown, accidents are as numerous as here, I think there are sufficient reasons for subjecting them to close inspection, forcing owners to adopt new and safe machinery, and use every effort in their power to prevent carelessness and exclude amateur operatives.

ART. VIII.—*Observations on the Comparative Advantages of Affording Obstetric Attendance on Poor Women in Lying-in Hospitals and in their own Homes.* By DENIS PHELAN, M.R.C.S., London; late P. L. Medical and General Inspector.

It seems, for many reasons, desirable to ascertain whether, in large cities and towns, such as London, Dublin, Glasgow, &c., society is as much benefited by affording obstetric attendance in lying-in or other hospitals to such poor women as wish to be confined in them as if it be afforded in their own residences—that is, in the residences of those who have a home in which they can be conveniently attended. Of course, for such exceptional cases as require careful medical watching or professional treatment the hospital is necessary.

The subject may be considered under these heads:—1. Which mode of attendance is the least expensive, assuming each to be efficient; and 2, by which is there less loss of life of mothers and children?

In respect to the comparative expense of hospital and of home attendance, we have the following data.

The Reports of the Board of Superintendence of Dublin Hospitals show that, in the seven years ended March, 1864, 9,039 labour cases, and 1,272 patients affected with chronic female diseases, were admitted into the Dublin Rotundo Lying-in Hospital, and that the expenditure amounted to £16,018, which sum (excluding the cost of buildings and furnishing such buildings) was an average cost of 31s. per head. During these seven years 617 extern labour cases were attended from the hospital, for which the expense is not given, probably because there is no officer paid for that particular duty, the cost of which is mixed up with the general expenditure.

During these seven years 3,496 labour cases, and 244 chronic patients were admitted into the Coombe Lying-in Hospital in Dublin, the expenditure being £5,600, which is an average cost of 29s. 10d. per head; but in this is included the expenditure on a dispensary for women and children, for which a medical officer is specially employed and paid. During that period 4,473 home labour cases were attended by the hospital medical officers and pupils.

If the cost per head be calculated on the totals attended in and out of hospital, that on the 10,928 cases attended by the Rotundo would average 29s. 3d. per head, and on the 8,210 attended by the Coombe, 13s. 6d. per head. This disparity of cost arises from the greater number of externs attended by the Coombe than by the Rotundo.

The expenditure of the London Royal Maternity Charity is stated to average £1,830 yearly, and the number of women to be 3,500 annually, which is an average cost per head of 10s. 6d. The attendance by this institution is exclusively extern.

I am not aware of any other published data to show the cost of home attendance on lying-in women. As that by the Rotundo Hospital is almost exclusively intern, the cost, of course, is proportionately high; the mixed system adopted by the Coombe reduces the average cost to a moderate sum, whilst the exclusively home attendance in the London Maternity is still less expensive.

MORTALITY OF MOTHERS IN CHILD-BED.

1. The Registrar-General reports that, in the year 1861, 886 died of metria (puerperal fever) in England and Wales, and 2,109 of the accidents of child-birth; in 1862, 940 died of metria, 2,137 of the accidents; and in 1863 the mortality was 1,115 from the former, and 2,483 from the latter. These returns show that the mortality from

metria in the first year was one in 781 births; in the second, one in 758; and in the third, one in 654. The average of the three years was one in 726. The mortality from the accidents of childbirth was one in 330 births in the first year, one in 333 in the second, and one in 255 in the third. The average of the three years was one in 306.

In 1861 the mortality from both causes was one in 235 births; in 1862, one in 231; and in 1863, one in 202; the average of the three years being in the proportion of one death to 223 births.

2. In the first of these years, 162 died of metria and 234 of the accidents, in London; in the second, 190 died of the one, and 245 of the other; and in the third year, 222 and 227 died. From metria the mortality in these respective years was one in 610 births, one in 531, and one in 464; and from the accidents, one in 424, one in 414, and one in 450. The mortality from both causes was in the proportion of one death to 234 births in the three years; in first as one to 250, in the second one to 232, and in the third one to 228.

3. In 27 cities and towns of the largest populations and trade in England, whose population is about 3,546,000, 162 died of metria and 427 from the accidents of child-bed in 1862; and in 1863 153 died of the former and 395 of the latter. The mortality from metria in 1862 was one in 761 births, in 1863 one in 800. From the accidents of child-birth it was one in 290 and one in 312. From both causes it was in the proportion of one to 209 births in 1862, one to 225 in 1863.

4. The Registrar-General for Scotland reports that 203 died of metria in that country, in 1861, and 130 in 1862, being in the proportion of one to 500 births in the former years, of one to 800 in the latter. From the accidents 308 died in 1861, and 305 in 1862, being one in 345 births in each year. From both causes the mortality was one in 207 births in the first year, one in 243 in the second.

5. In 1861 the mortality from metria was 84, and in 1862 it amounted to 49, in seven of the most populous and trading cities and towns in Scotland, whose population is about 886,000; and that from the accidents of child-birth was 108 in the first year and 98 in the second. From both causes the mortality averaged 178 annually, and was in the proportion of one to 170 births. In Glasgow 57 died of metria, in the two years, and 85 of the accidents, which was one in 227 births annually.

There is yet no published account of the mortality in Ireland from these two causes, but the Registrar-General has published lists of it for the years 1864 and 1865, in the registration district of Dublin, which includes Kingstown, Blackrock, Donnybrook and Rathmines; and, as my object is to compare the mortality in large and dense populations, the returns for the Dublin district will answer that purpose.

6. The population of that district is 314,409. In these two years there were 72 deaths from metria and 48 from child-birth—total, 120. The births averaged 7,952 annually; the mortality from both causes was one to $132\frac{1}{2}$ births; from metria one in 221, from the accidents one in 331.

The mortality of mothers in child-bed in lying-in and other hospitals is now to be considered.

1. Mr. Simon, the medical officer of the English Privy Council, gives the following information in the *Sixth Report on Public Health*:—"In Paris there are, besides one large lying-in hospital (La Maison d'Accouchement), in all the general hospitals beds (numbering from 14 to 52) set apart for the delivery of females. In the year 1862 2,204 births took place in the Lying-in Hospital; and 166, or one in $13\frac{1}{4}$, died; 4,764 were confined in the other eleven Parisian hospitals; and 310, or one in $15\frac{1}{3}$, died. The mortality from metria was in the proportion of one to $15\frac{2}{3}$ births in the Lying-in Hospital; in the other hospitals, as one to $18\frac{1}{2}$.

2. "In the fifteen years ended 1859, 8,036 women were delivered in the St. Petersburg Midwives' Institution, of whom 306, or one in $26\frac{2}{3}$, died. In these 15 years 25,711 labour cases were admitted into the hospitals of that city, and 1,117 of them died in child-bed, which was one in 22.

3. "During the 24 years ended 1856, 4,960 deliveries took place in the London York-road Hospital, and 146, or one in 31, died; in two of these years 188 women were delivered in it, and 34 died, which was a mortality of one in $5\frac{1}{2}$ births. The principal cause of death was puerperal fever. Puerperal fever has occurred in this institution since 1856; the last outbreak was connected with scarlet fever, in 1861. The place had to be closed for three or four months.

4. "During five years 10,000 deliveries were effected in the London metropolitan hospitals, of whom 129, or one in $77\frac{1}{2}$, died."

5. "On summing up the results of several continental lying-in hospitals (the years are not stated), Dr. Barnes found that, out of 14,253 deliveries, 247 women died, which was one in $56\frac{3}{4}$."

6. We learn from Sir William Wilde's valuable work on the Vienna institutions that the lying-in hospital there admitted 4,453 women in one year, and that 179, or one in 25, died—a mortality which he attributes to puerperal fever. "This affection," he observes, "makes fearful ravages in the hospital annually. It is not considered by the medical men as infectious, and therefore no precaution is ever taken to prevent its spread, by cleaning, whitewashing, or shutting up wards where it has particularly prevailed for any length of time; nay, more, I have seen a newly-delivered woman placed in a bed, yet scarcely cold, in which a death from puerperal fever had taken place not two hours before."

This institution, though the most celebrated and extensive of its kind in Europe, must, under such management, be a curse, instead of a blessing, to the Viennese. In the nine years ended 1838, it admitted 32,679 non-paying labour cases; the mortality is not given; but if it were, an average of one in 25, as in 1838, it is likely that, at least, 1,200 women must have died in it more than would have died had they been attended at home, or had there been no lying-in hospital in Vienna.

7. "In the years 1861 and 1862 705 women were confined in the Glasgow Lying-in Hospital, and 13, or one in $54\frac{1}{2}$, died. This hospital contains only 24 beds."

8. "During the seven years ending 1862, 1,092 labour cases were conducted in the Liverpool Lying-in Hospital, and eleven proved fatal to the mother," which was a mortality of one in 99. "This hospital is small, clean, and apparently well conducted, and is intended for the reception of a small number of cases of diseases of the female organs, and for the delivery of *respectable* married females."

9. The Dublin Rotundo Hospital Statistical Table shows that 8,224 births took place in that institution in the seven years ended 1864, and that 252 mothers died, which is one in $32\frac{2}{5}$ births. During these seven years 3,142 deliveries took place in the Coombe Lying-in Hospital, and 45, or one in 70, died.

MORTALITY OF MOTHERS UNDER HOME ATTENDANCE.

1. Mr. Simon states, on the authority of Dr. Barnes, that, during five years, 18,751 women were delivered by the Royal Maternity Charity in London, and that of these 56, or one in 334, died.

2. Mr. Simon also cites the authority of Dr. Hugenberg of

St. Petersburg, that in 15 years 207,582 women were delivered in that city at their own homes, with a mortality of 1,453 mothers, which was one in 143.

3. In the years 1861 and 1862, 729 women were attended at home from the Glasgow Lying-in Hospital, and 10, or one in 73, died.

4. During the seven years ended 1864, 617 women were attended from the Dublin Rotundo Hospital, and ten, or one in 62, died.

5. During the same seven years the Coombe medical officers attended 4,473 at their homes, with the loss of 20 mothers, or one in 223.

Since the foregoing was written I have seen the article, "*Etude sur les Maternites*," in the October number of the *Annales d'Hygiène Publique*, in which the editor reviews a work by Dr. Leon Le Fort, and shows that he and others have lately published a vast mass of carefully-collected returns to prove three propositions.

"Considered altogether, Dr. Leon Le Fort's work is composed of three *essential* parts, or propositions.

"1. The women who are confined in the hospitals and maternities not only die there in much greater numbers, but die in a quite unusual proportion compared with those who are confined in their own dwellings.

"2. The cause of this frightful mortality must be attributed to puerperal fever, *and it is by contagion that this destructive scourge exerts its ravages.*

"3. It is absolutely necessary to take serious hygienic measures; and if the malady cannot be prevented from breaking out, it is, however, possible to oppose barriers to it, and to say:—'Thou shalt go no further.'

Amongst the voluminous returns referred to by the editor are the following:—

"Of 888,312 women confined in maternities or in hospitals, 30,954 died; and of 934,781 deliveries, effected in the towns, 4,405 were followed by death. The mortality was, in the first case, one woman out of 29 confined; in the second case, it was only one out of 212."

These returns appear to confirm Mr. Simon's statement that "the

mortality in lying-in hospitals is almost invariably greater than among women delivered at their own homes;" and a careful inquiry respecting the cause or causes of this certain relative mortality will show that it is more immediately attributable to the greater occurrence of puerperal fever, and of other contagious febrile diseases in hospitals, than in home attendance.

It would be reasonable to ask, how it happens that a greater relative proportion of women die in lying-in and other hospitals, in which great attention is paid to ventilation and other hygienic conditions, and over which highly educated and experienced medical men preside, than die at home, in residences many of which are comparatively wretched and ill-ventilated, and with far less comfort to carry them through their confinement. Until recently the cause, or causes, or the extent of their effect, was not, I believe, well known. The fact is thus accounted for by Mr. Simon in the before-mentioned report. He observes:—"The admission or exclusion of infectious diseases forms a very important item in regulating the mortality of hospitals.

"There are certain affections, especially eruptive fevers, which are capable of being conveyed from the infected to the healthy by the breath, or other emanations or secretions of the body, and whenever or wherever (except under certain conditions) anyone comes under their influence, he runs the risk of being infected by them; all this, of course, is a mere truism, but it is important to enunciate it clearly. Such a spread of disease proves, of course, the presence of a poison; it shows, what everyone would admit, that *the admission of any infectious case involves a risk; it raises the question as to the propriety of exposing anyone to such a risk who comes to a hospital for cure.*

"All surgeons attached to large metropolitan hospitals are aware that their operation cases are apt to be occasionally carried off by the supervention of erysipelas, pyemia, or hospital gangrene, or some allied unhealthy form of inflammation. They know well that they may go weeks, months, and even years, without losing a patient from any of the above causes, though occasionally an isolated case may occur, and that now and then some one of these secondary diseases prevails for a time, endemically, attacking operation case after operation case, until the ward, or wards, which have been the seat of the disease, have been emptied, and thoroughly ventilated and cleansed; and that, *after apparently ample precautions of this kind have been carried into effect, the re-occupation of the*

emptied beds may be the signal for the renewal of the disease. It is generally considered that the occurrence of these secondary forms of disease, and especially their frequent recurrence, or their prevalence in an endemic form, is an indication of unhealthiness in the hospital or in that part of it in which they manifest themselves. There can be little doubt that the development and spread of these diseases are associated with defective hygienic conditions, but there can be little doubt that these sanitary defects are not the sole cause of the outbreak of these unfortunate complications; for it seems to be a tolerably well ascertained fact that, in places where they become endemic, there must be (not merely to allow of their spreading, but in order to beget them), an accumulation of open sores, producing what is termed a *traumatic* atmosphere.

“ We have, finally, to say a few words with regard to the results of midwifery practice in wards in hospitals specially devoted to the reception of pregnant women. The lying-in hospitals of England and Scotland are, for the most part, small and insignificant institutions; indeed, the only lying-in hospital of much importance in Great Britain is the Rotundo Hospital, Dublin. In this admirable institution—admirable alike for its construction and its arrangements—the results of midwifery practice are not unsatisfactory. Though occasional cases of puerperal fever are not uncommon in the Rotundo, epidemic outbreaks are comparatively rare; and when they do occur, are regarded as due rather to some epidemic influence than to spread by contagion. So strongly is this opinion held that *sporadic cases arising in the hospital are never (except when they are about to die) separated from the other labour cases; and it is asserted that no ill consequences whatever ensue from this practice.* In the epidemic referred to (that of 1861-2, in which the mortality was 8·3 per cent., 46 were attacked with puerperal fever, and 28 died. and eight were attacked with scarlet fever, and seven died) there was evidently some close connexion between the prevalence of puerperal fever and the prevalence of scarlatina. The outbreak, therefore, furnishes an example of the well-established fact that scarlet fever, typhus, and other exanthemata are not only peculiarly apt to attack puerperal women exposed to their contagious influences, but prove peculiarly fatal to them, and that these poisons constitute a fruitful source of contagious forms of the so-called puerperal fever.” Mr. Simon continues:—“ But though in Dublin it seems to be questioned, there can, we believe, be little doubt that puerperal women have many points of resemblance to operation

cases, and that the presence of a *traumatic atmosphere*, such as is created by the accumulation of large numbers of women recently confined, is attended with many of the dangers which certainly attend a similar atmosphere in a surgical ward. We do not mean that the traumatic atmosphere in itself creates disease, but we believe that puerperal women are peculiarly susceptible to those poisonous influences which, among surgical patients, produce erysipelas, pyemia, and the like; and that when those poisonous influences are present, the traumatic atmosphere favours their injurious operation. It need scarcely be said that a very large proportion of puerperal fever cases are pyemic, originating in some inflammatory condition of the uterine walls, and that it has been over and over again proved that this particular form of the disease may be conveyed by the medical attendants, by the nurses, and by dressings, and the like.

“Now, the presence in any institution of the former variety of disease, that relating to exanthemata, must depend on the entrance into that institution of some form of exanthematous poison. It is an accident, therefore, which can only be occasional, but which can scarcely fail to occasionally occur; *and when it does occur, in large institutions specially, is extremely likely to produce grievous consequence.*

“It is certain that this disease (puerperal fever), like pyemia, in surgical wards, becomes at times, and from similar causes, *endemic* in hospitals, and that decided measures are then needed for its eradication. There is no doubt that sporadic cases of puerperal pyemia occur in other British maternity hospitals besides the Rotundo; but these institutions are on so small a scale, and can be so soon emptied, when any sign of danger manifests itself, that anything amounting numerically to an epidemic ought scarcely to arise; still, we believe that all the London maternity hospitals have had, from the prevalence of puerperal fever, to be closed on more than one occasion. The experience of the York-road Lying-in Hospital, more, however, formerly than of late years, has been particularly unsatisfactory. In Paris it is a striking fact, that the results are (for almost every hospital) analogous, in many respects, to those of surgical operations. The appended tables show how high the mortality among lying-in women is universally in the Paris hospitals, and, at the same time, how very largely this high mortality depends on ‘puerperal fever and puerperal peritonitis,’ which terms, doubtless, signify (since deaths from eruptive fevers are distinguished) essentially pyemia.

“It appears, however, that the mortality in lying-in hospitals is almost invariably greater than that amongst women delivered at their own houses. A mortality of between one and two per cent. is, in ordinary years, that occurring in the Rotundo. In small and well-conducted establishments, like those of Liverpool and Glasgow, the mortality is usually about the same; and, on the whole, that of the lying-in hospitals in London is probably not very different. A death-rate of one or two per cent. may not appear very alarming, but when we know that the per centage of deaths out of hospital is considerably less than this, often not exceeding 0·3, or even 0·2 (one in 300, or one in 200), we are led to suspect that the excess of mortality above these numbers may depend upon hospital influences.

“Whenever surgical cases presenting open sores are received, hospital diseases, such as erysipelas, pyemia, and phagedena, are liable to arise. The liability of these affections to originate and to spread is considerably influenced by concentration of traumatic atmosphere.

“Puerperal women are exposed, in hospitals, to two dangers. Like patients after surgical operations, and for the same reasons, they are peculiarly susceptible to the influences of those conditions on which pyemia and erysipelas depend; they are also highly susceptible to the poison of contagious fevers.

“The accumulation of puerperal women in a ward creates a traumatic atmosphere, with all its risks, and furnishes material on which the poison of an infectious disease, accidentally introduced, acts with terrible violence. *But labour is a natural process, and only in a comparatively small number of cases calls for the special exercise of skill in nursing or medical treatment. There are, therefore, generally, in the case of puerperal women, none of those special objects to be gained by becoming the inmates of a lying-in charity, which the diseased and maimed seek by admission into general hospitals.*”

These statements and opinions appear to be fully confirmed by an account which Dr. Telford, one of the Rotundo Hospital assistants, gives, in the October number of the *Dublin Medical Press*, in reference to the mortality caused by puerperal fever in that institution in the month of April last. He states:—“The epidemic of puerperal fever which visited the Lying-in Hospital in April last is deserving of notice. The suddenness of its invasion,

the extreme rapidity with which most of the cases terminated, and the great mortality in the numbers attacked—16 dying out of 17—mark this outbreak as one of very unusual virulence. The hospital, for the time of the year, was in a fair state of health. During the month of March there were 117 cases delivered, and five died.” “On the 23rd of April a patient, who had been delivered on the 8th inst., died. On the evening of the 23rd a woman, who had been confined the day before, had a rigor, and died next day. In the same ward with the last patient were four others, three of whom were attacked, all died, none of them living more than twelve hours after the first symptom.

“Thus, out of a total of 25 patients who were in the lying-in wards at the time of the outbreak, seventeen were attacked and sixteen died. We had a patient in the chronic ward suffering from membranous dysmenorrhea; she was carried off in three days also.”

The hospital was then closed, and “was whitewashed, fumigated, and painted. Since then we had 145 deliveries, and only one death, from placenta previa.”

Dr. Telford continues:—“A deal has been written as to whether puerperal fever is infectious or not, and very opposite opinions held by some of our most distinguished physicians. In its epidemic form we believe that almost every patient brought within its influence will be attacked, but that does not prove it to be infectious, as one can hardly call it infection where such a number of patients are attacked simultaneously. It is *the custom in this hospital to have patients suffering from this disease in the same ward with others; in fact, we never remove them until we anticipate a fatal termination*, and then more for the purpose of saving the other patients’ feelings than to guard against infection. Still we never find the disease communicated from one to the other.

“The poison which produces puerperal fever has been supposed by some to be identical with that which produces erysipelas. This was, to a certain extent, borne out by one of our wardmaids, who had been in constant attendance on the patients, being attacked with erysipelas, of which she died. I may also state that erysipelas was very prevalent in the city at the same time, and, in some of the surgical hospitals, *the surgeons were obliged to defer any operation proceedings.*”

Whilst giving Dr. Telford full credit for his valuable paper, and for his belief in the opinions that are therein expressed, I hope a few remarks on some passages in it will not be considered uncourteous or inconsistent with the subject. I would ask how can a lying-in hospital be considered in a state of fair health if the five deaths that occurred in it in the previous month, or the most of them, died of puerperal fever? Dr. Telford doubts that the disease was caused by infection; which opinion appears to be that of the medical authorities of the hospital, as they "leave patients suffering from puerperal fever in the same ward with others, and never remove them till they anticipate a fatal result." Then, as it may be inferred that some were affected in March, the disease cannot have been sporadic (which is defined in Dunglisson's Medical Dictionary to be "diseases which supervene indifferently in every season and situation, from *accidental* causes, and independently of any *epidemic or contagious* influences"); and if it be neither contagious nor sporadic, it must have been endemic (or owing to some peculiarity in the situation or locality). But it is difficult to understand how an institution which is truly described as "admirable in construction and in its arrangements," and in which great attention is paid to cleanliness, ventilation, &c., can have been so very malarious that seventeen lying-in women, separated as they must have been in different wards, would become so suddenly affected with one particular febrile disease; and, as I have frequently gone through this fine institution, officially and otherwise, and have always found it in a satisfactory state, I cannot believe that the puerperal fever described by Dr. Telford was either sporadic or endemic. This is a question of so much importance in the safe conducting of lying-in hospitals, that if Mr. Simon's opinion, as well as that of "some of our most distinguished physicians," as admitted by Dr. Telford, that puerperal fever is contagious, be well founded, it follows that if an infectious disease spreads in the wards of the Rotundo, or any other hospital, there is a *risk* that others coming under its influence will be affected with it; and, therefore, "that the question is raised as to the propriety of exposing anyone to such a risk who comes to an hospital for cure," or merely to be confined, as it may be assumed was the case with the parties described by Dr. Telford.

In reference to the admissions into lying-in hospitals. Mr. Simon observes:—"It seems to us, from considerations which are discussed in the section on the "Health of Hospitals," that *they are undesirable*, and that the appropriation of wards to the reception of

puerperal women in general hospitals is especially to be condemned. It is, of course, possible that certain circumstances may (*as is said to be the case in Dublin*) render a lying-in hospital, like a workhouse, a matter of necessity; and it is also possible that certain cases in which special dangers are to be apprehended at the time of delivery, would be safer in an institution where they could be more carefully watched than at home; but these are exceptional cases, and do not furnish any valid argument against the general opinion we venture to express."

It would seem, from the words ("as is said to be the case in Dublin") in the above quotation, that Dr. Bristow and Mr. Holmes, who in 1863 examined and reported to Mr. Simon on the Dublin hospitals, understood that extensive hospital accommodation for the delivery of lying-in women was a *necessity* in Dublin, and that without it much loss of life and other serious consequences must result. It might have been difficult to confute that view before the workhouses were established, as there were no reliable data to show what amount of accommodation would have been sufficient; but now, when every woman of ill character, and every woman who has no fixed residence, can get admission as a pauper into the workhouse, that *necessity* does not seem to exist, beyond the exceptional classes before alluded to.

The large number of deliveries that have taken place in the Rotundo has been adverted to; and it would seem, from an observation by Mr. Simon, that Dr. Bristow and Mr. Holmes understood that circumstance to be owing to the poverty of the women of the working classes in Dublin, which poverty rendered them unable to meet the expense attendant on their lying-in. But it cannot be reasonably supposed that one-third, or one-fourth, of the child-bearing women of Dublin were, or are, so poor as to be unable to bear the ordinary expenses of their confinement. In the seven years of Dr. Labatt's mastership, for instance, the enormous number of 21,867, or more than 3,000 yearly, were delivered in the Rotundo, which was more than one-third of all the births of rich and poor in Dublin; and though latterly the number is much less, at least one-seventh of all the deliveries in Dublin have taken place in the Rotundo in the last ten years. Every intern pupil of the hospital knows that many, in sufficiently comfortable circumstances to meet the expense of their lying-in, go into that institution, partly to be under good professional treatment, but partly, also, to save the

expense of their confinement, which they are enabled to effect by the facility with which they are admitted into it, as will be perceived from the following evidence, given by the then master, Dr. Shekleton, before the House of Commons Committee on Dublin Hospitals, in 1854:—

Q. "Do you admit every woman who presents herself at the door for admission?" A. "Yes, if she come in labour to the gate, there is no question asked."

Q. "Do you ever ask any questions with respect to women, if they come more than once, to ascertain whether they really are objects of charity?" A. "No."

Q. "If they were to come half-a-dozen times would you make any enquiry?" A. "We never refuse a woman admission; *we have no power to do so.*"

Q. "Whether she is poor or not?" A. "No."

Q. "You take it for granted that she is poor?" A. "Yes."

Q. "There is no check upon a woman coming into the hospital and delivered of an illegitimate child?" A. "None whatever."

Q. "Do you ever take any steps to inquire into the condition or life of the husband?" A. "Never: R. C. clergymen generally recommend the patients."

Q. "Is there no means of checking a woman coming into the hospital regularly to be delivered of an illegitimate child?" A. "None whatever."

Q. "The same woman might come in six times in a dozen years and be delivered of an illegitimate child?" A. "Certainly."

Q. "If you discovered that, would you refuse her admission?" A. "No; we have no power of refusal."^a

Q. "Do you ever refuse admission to a woman who comes, in labour, to the hospital?" A. "Never."

Q. "Is the hospital restricted to married women?" A. "We never make inquiries; they all pass as married women whether they have husbands or not."

During my pupilage in the Rotundo, in 1813, many of this non-paying class were admitted, whose appearance and dress indicated no necessity for gratuitous hospital treatment; and many, too, died in it of puerperal fever which, it is very likely, most

^a The hospital charter appears to give full authority to the governors to make a by-law to meet the case.

of them might have escaped had they been confined at home. Dr Shekleton's evidence shows that the abandoned, as well as the virtuous, could take advantage of the hospital, and that no rule or by-law existed to check the repeated admissions of women of the former class, and I am not aware that any now exists. It is true, as before observed, that when, and long after, the hospital was founded there was no workhouse to which that immoral class could have recourse when about to be confined; but for many years before that evidence was given they were admissible into all the Irish workhouses as paupers.^a The *necessity*, therefore, that existed for the admission of this immoral class had long ceased in 1854, and their admission cannot, or could not, be fairly deemed an act of necessity nor of charity. Viewing the subject in a moral sense, it appears likely that such women would be more checked in their career by being compelled to have recourse to the workhouse, where they would have less, though still sufficient, comforts than if received into a superior institution where the stigma of their habits is much concealed. And in reference to the charter of the Rotundo Hospital, it is evident that it was not intended that the hospital should be available for the class in question. The words are:—"In many parts of our said kingdom, and especially in the city and suburbs of Dublin, there are always many poor and distressed women great with child, who, by the sickness, death, absence, neglect, or extreme poverty, *of their husbands*, wholly depend on their own daily work for even common necessities, and are, on lying-in, frequently both themselves and their infants lost, not only by the difficulty of obtaining the care of some skilful person, but even through the want of such covering, lodging, and sustenance as are necessary for women in that condition, many instances of which could be produced, especially in the case of wives and widows of soldiers and sailors of our army and navy."

These words, I think, appear to imply that the Rotundo was intended to receive one class chiefly—such *wives* and widows as, from any of the circumstances so minutely described in the charter, are unable to provide the necessities attendant on their confinement. I believe that, on the contrary, many who are fully able to meet such expense are admitted. I have known the wives of servants,

^a During the six years ended 1865, 16,310 births took place in the Irish workhouses, and 124 women, or one in 133, died. The mortality was highest in 1863—one in 90; and lowest in 1864—one in 190.

both husband and wife having good wages, and money to spare, to avail themselves of the Rotundo; and I infer, from the words of the charter, that neither such parties, nor those of the immoral class, were intended to be relieved in it, unless when an exceptional case might require careful medical watching and professional treatment.

Many doubt that it is judicious to give too great facilities to women of this comparatively comfortable class to avail themselves of a public charity, as it is found that it also gives them the habit of craving from their more wealthy neighbours that with which, by thrift, they could readily supply themselves, and that it lessens that spirit of independence and foresight which it is desirable to encourage.

Another view of this subject appears to require the serious consideration of lying-in women, and of the authorities of hospitals into which they are admitted, of which, perhaps no better illustration could be given than is afforded by Dr. Telford's letter. He states:—"Thus, out of a total of twenty-five patients who were in the lying-in wards at the time of the outbreak, seventeen were attacked, and sixteen died." If, suppose eight of these sixteen could have afforded to meet the moderate expense of being confined at home, under professional attendance from one of the lying-in hospitals, or by the dispensary medical officers, the whole of them, or the greater part, would probably have been saved; for, I find by the Registrar-General's Return, that the mortality from metria, for the five weeks ended 5th of May, was only seventeen, whereas that returned by Dr. Telford is sixteen. The Rotundo was closed on the 23rd April; and there is no return of deaths from puerperal fever by the Registrar-General in the week ended 5th May. It would seem, therefore, that only one died of that disease in the registration district outside the Rotundo—at least, if the returns made by the several registrars be correct.

When loss of life is concerned, another illustration of the caution with which those who can afford the cost of home confinement (unless for some exceptional reason) should desire to be confined in hospitals. During the three first years of Dr. Labatt's mastership in the Rotundo, 10,248 deliveries took place in it, and only fifty-seven women, or one in 180, died; but in the next four years of his office, 252 died out of 11,628, which was one in 46. This higher mortality must have been chiefly caused by puerperal and other fevers; for it is not likely that an able midwifery practitioner

would have had more deaths from the accidents of childbirth in the last years of his mastership than in the early years; but the contrary, as his experience had much increased. Now, if we only assume that about the half of that 11,628 were of the comfortable class that could afford to be confined at home, and had been confined there, eighty-six mothers would have been saved, even though the mortality were one in 150; but even if it were so high as one per cent., sixty-eight lives would have been saved. Several similar instances could be given.

We have the high authority of Drs. Sinclair and Johnston, who were assistants in the Rotundo, and who have published a valuable work on practical midwifery, that even those who have had natural deliveries were not safe from puerperal fever. They state that sixty-seven mothers died in the Rotundo, in their time, "whose deliveries had been purely natural," and that forty-one of them died of puerperal fever. Had these been confined at home, some might, perhaps, have had that disease; but it is not likely that so many would; and, as their labours were natural, they did not go to hospital on account of malformations, or from apprehension of difficult labours.

The high mortality in lying-in and other hospitals, and its frequent recurrence, will be perceived by the Table at page 92, which shows that in the Rotundo it has been very low in some years, and again very high; it has occasionally continued low for two, three, four, five, and six years, and again become continuously high for five years. It will be seen that in some years the mortality ranged from one in 145 deliveries to one in 233; and that in several others it was from one in 52 to one in 14. These marked extremes are scarcely explicable, unless on the supposition of the recurrence of puerperal or other febrile diseases in the hospital; as it is not likely that under the able practitioners who attend it, and have always done so, such a high mortality would have been caused year after year by the accidents of childbirth, such as ruptured uterus, hemorrhage, &c., &c., which usually influence the mortality of lying-in women.

Applying the foregoing returns and observations to the Rotundo Lying-in Hospital, as it is the most commodious in Great Britain or Ireland, and has the most ample and certain funds, the question arises, what causes this occasional, but frequent high mortality in an institution in which cleanliness and ventilation are carefully observed, the comforts of the patients well attended to, and over

which very able and eminent midwifery practitioners preside? All these elements in the condition of the hospital would naturally indicate a certain low mortality, at least as low as that which takes place outside, where these comforts and medical superintendence are not so much available. The answer would appear to be, that the chief cause is that, which is stated by Mr. Simon and Dr. Le Fort to produce a higher mortality of parturient women in lying-in and other large hospitals, namely, the occasional introduction into them of contagious febrile diseases, particularly scarlatina and typhus fever, and thus, though indirectly, puerperal fever is produced. The introduction of such contagious diseases into the Rotundo is the more likely to take place, as fever usually, and scarlatina frequently, prevail in Dublin; and it may be occasionally expected that a woman who comes to the hospital to be confined is not only then in actual labour, but is also affected, though not perceptibly, with fever or scarlatina.

The published reports of the board of superintendence of Dublin hospitals throw much light on this subject. Previously there were no data to show what proportion of the mortality that took place in either of the Dublin lying-in hospitals was caused, year after year, by puerperal and other fevers, and the same was the case in respect to the extern attendance given by these institutions. Those reports show that in the seven years ended March, 1864, of 258 woman that died in the Rotundo, 131 died of puerperal fever, and of 45 that died in the Coombe, 15 died of it; and that this disease was a chief cause of the mortality in the Rotundo in each of the seven years.

As there is no doubt that the Rotundo authorities have considerably exerted themselves to improve the sanitary condition of that fine institution, and yet that a high mortality, chiefly from puerperal and other fevers, which are preventible diseases, takes place in it, it may not be amiss to observe that for several years the rate of that mortality has increased in it. Before 1854 the mortality ranged from one in 214, the lowest in any year, to one in $32\frac{1}{2}$, the highest—the average of the previous long period being one in 82; since 1854, included, the mortality has ranged from one in 64, the lowest in any year, to one in $13\frac{1}{2}$, the highest in any; the average of this twelve years being one in $37\frac{1}{3}$ —facts, of which, possibly, the hospital authorities are not aware, and which can be only ascertained on a careful examination of the statistics of the institution.

We have also evidence in the returns of the board of superintendence that infectious fevers, as scarlatina, typhus, &c., are occasionally introduced into the Rotundo, and that even pyemia has prevailed there; and it is difficult, indeed, to conceive how, under the arrangement described by Dr. Shekleton, their occasional introduction can be prevented. As before observed, such women, on application, may be in the incubation stage of scarlatina, or of typhus; and even if her rejection were to depend on that circumstance being perceived by the master or assistant, he might not *then* be able to ascertain it. She is therefore admitted, and is placed in a ward along with other labour cases; it is only in a day or two that she is found to be ill of scarlatina or typhus, and then, whether delivered or not, she is in an unfit condition to be removed to a fever hospital. But even if so transferred, or if put into a separate ward intended for patients affected with contagious diseases, she has been long enough among the labour cases to introduce a poison which, in the existing condition of the labour patients, causes puerperal fever, a disease, of which Dr. Leon Le Fort, as well as Mr. Simon, observes that "*it is by contagion that this destructive scourge exerts its ravages.*"

Whether the puerperal fever that caused such mortality last April was preceded by the introduction of any contagious disease does not appear, but it is certain that a still greater mortality was connected with scarlatina in the year ended March, 1862, in which 2 labour cases died of typhus fever, 11 of scarlatina, 4 of pyemia, and 34 of puerperal fever. Referring to the mortality of that year, Dr. Bristow, who examined the hospital, observes:—"The outbreak furnishes an example of the well established fact that scarlet fever, typhus, and other exanthemata, are not only peculiarly apt to attack puerperal woman exposed to their contagious influence but are peculiarly fatal to them, and that these poisons prove a frightful source of contagious forms of the so-called puerperal fever."

It seems to follow from the foregoing data and observations that one mode of lessening the mortality of lying-in women in hospitals is, to limit the admissions to the classes that more particularly require hospital treatment, and to give extern attendance to those that do not. This opinion has been expressed in reference to the Rotundo by four different boards of commissioners, which have examined and reported on it, as is expressed by the commissioners appointed to report on the Dublin hospitals in 1855.

They observe—"We are of opinion that the practice of attending on patients beyond the walls of the hospital is not sufficiently followed in this institution. On this point we concur in the opinion which was expressed by the Board of Health in 1820, and which is referred to in the Reports of the Commissioners of 1830 and 1842. We consider this subject to be worthy of more attention than it has hitherto received, and that the principle of attending on extern lying-in cases, *especially, during the prevalence of puerperal fever*, should be acted on as extensively as possible." The reports of the Board of Superintendence of Dublin Hospitals show that this recommendation has not been much acted on, as it appears by them that, in the year ended March, 1862, of 978 labour cases that were admitted, 80, or one in $12\frac{1}{2}$ died in the hospital—39 of puerperal fever, and that only 144 had home attendance; and that in the succeeding year 1,040 labour cases were admitted, and 41, or one in $25\frac{1}{3}$, died, of whom 29 died of puerperal fever, and that only 2 had extern attendance in the year.

THE MORTALITY OF CHILDREN IN LYING-IN HOSPITALS.

We learn from Sir William Wilde's work, before quoted, that out of 23,322 births which took place in the Vienna Lying-in Hospital, 1,482 children, or one in $15\frac{2}{3}$, died before the ninth day, and, that, of 4,453 births in that institution, in 1838, there died 200 children, which was one in 21 births.

The census report, of 1851, contains a table, which shows that of 35,131 births that had taken place, in ten lying-in hospitals, in Ireland, 2,258 children died, which was an average of one in $15\frac{1}{2}$ births.

The census report, of 1861, shows that in the ten years then ended, 25,249 children died in Ireland, under one month old, which is a mortality of one in 55 births. I am not aware of any return that shows the mortality of infants under, about, ten days old, in the whole population; but if it be one in 55 at a month, it must be a lower mortality within ten days, as many die within the other twenty days.

The printed statistical table, of the Rotundo Hospital, shows that 177,708 children were born alive in that institution and in the old hospital; and that 6,745 of them died in it, that is, in the few days, generally 8 or 10, during which the mothers remain in it. This was a mortality of one in $26\frac{1}{3}$ children born alive. The proportions in which this mortality took place under the different

masters is very remarkable as will be perceived by the following table.

Table showing the Mortality of Children Born in the Rotundo Lying-in Hospital, under the Different Masters, and the Proportion of such Mortality to the Number of Children Born Alive.

Masters	No. of children born alive during the Master-ship	No. of still-born children	No. of children died in hospital	Proportion of children died in hospital	Ranging from () to ()	Proportion of still-born children to all children born in hospital
1st	891	46	115	1 to $5\frac{3}{8}$	1 in 8 to 1 in $5\frac{3}{8}$	1 in $26\frac{1}{2}$
2nd	3,657	197	708	1 to $5\frac{1}{8}$	1 in $6\frac{1}{2}$ to 1 in 5	1 in $19\frac{2}{3}$
3rd	4,552	258	892	1 to $5\frac{1}{16}$	1 in $6\frac{1}{2}$ to 1 in 4	1 in $18\frac{2}{3}$
4th	5,735	410	921	1 to $6\frac{1}{4}$	1 in 8 to 1 in 5	1 in $22\frac{1}{2}$
5th	6,823	580	553	1 to $12\frac{1}{2}$	1 in 25 to 1 in $7\frac{1}{2}$	1 in $17\frac{1}{2}$
6th	10,294	600	421	1 to $24\frac{1}{2}$	1 in 36 to 1 in 22	1 in $18\frac{4}{5}$
7th	10,993	974	401	1 to $23\frac{1}{2}$	1 in 40 to 1 in $20\frac{1}{2}$	1 in 23
8th	14,096	1,063	336	1 to 42	1 in 70 to 1 in 25	1 in $15\frac{1}{2}$
9th	17,988	851	403	1 to $44\frac{1}{2}$	1 in 62 to 1 in 28	1 in 18
10th	21,116	1,535	399	1 to 53	1 in 61 to 1 in 41	1 in $14\frac{2}{3}$
11th	12,269	777	154	1 to 79	1 in 136 to 1 in $61\frac{1}{2}$	1 in 17
12th	15,627	1,017	157	1 to 99	1 in 176 to 1 in 75	1 in $16\frac{1}{3}$
13th	12,672	651	73	1 to 172	1 in 469 to 1 in 93	1 in $20\frac{1}{3}$
14th	13,035	861	151	1 to 86	1 in 193 to 1 in $50\frac{2}{3}$	1 in 16
15th	12,828	1,044	340	1 to $37\frac{2}{3}$	1 in 66 to 1 in 32	1 in $13\frac{1}{3}$
16th	8,707	711	312	1 to 28	1 in 44 to 1 in 20	1 in 13
17th	2,997	263	78	1 to 38	1 in 56 to 1 in $22\frac{1}{4}$	1 in $12\frac{1}{2}$

Why that mortality should be from one in 4 to one in $7\frac{1}{2}$, under the five first masters, and only one in 86, 99, and 172, under others, might suggest grave considerations. The subject appears to deserve more attention than, so far as I am aware, it has

yet received. Dr. Clarke's improvement in the hospital ventilation would seem to have reduced the mortality from one in $12\frac{1}{2}$ to one in $24\frac{1}{2}$; but it appears remarkable that the mortality of women under him was one in 87, being in the time of his predecessors only one in 131, and in that of his successor one in $93\frac{2}{3}$. It would be curious if an improvement that would be beneficial to infants should not also be useful to the mothers.

STILL-BORN CHILDREN.

We also learn from Sir William Wilde's work that 278,613 births that were registered in twelve cities in the Austrian dominions, and that 9,697 of the children were still-born, which was an average of one in $36\frac{2}{3}$ births. The proportion ranged from one in $59\frac{3}{4}$ in one district to one in 21 in another. But in Trieste it was so low as one in 155. In the Vienna Lying-in Hospital 939 were still-born, in 23,413 births, which was one in $24\frac{9}{10}$.

The Rotundo statistical table shows that 186,209 births took place in the old hospital and in the Rotundo, up to the end of 1864, and that 11,289^a were still-born, which was in the proportion of one to $16\frac{1}{2}$ births, ranging from one in $22\frac{1}{3}$, under one master, to one in $12\frac{1}{3}$ under another.

A question of considerable importance cannot be overlooked, namely, by which mode of attendance is medical and obstetric science best promoted. Doubtless, were the advantages of both equal, or nearly so, the greater and the more exact experience obtained in hospitals would decide in their favour, but, if there be far greater loss of life attendant on that mode, the question cannot arise. That there is far greater loss of mothers and of children in hospitals is now an admitted fact; but, though a much greater proportion may get home attendance, there will still be a sufficient number (of the exceptional cases that require hospital treatment) to afford ample materials for the instruction of pupils, and for affording information to the medical attendants.

P.S.—I beg to observe, that all the data and other sources of information referred to in this paper were obtained from published documents, which are as accessible to others as they are to me.—D. P.

^a The total is 10,447, but 11,289 is the number.

Statistical Table, referred to in page 86, 3rd paragraph :—

Year	No. of Births	No. of Women Died	Proportion of Deaths to Births	Year	No. of Births	No. of Women Died	Proportion of Deaths to Births
1760	556	4	1 to 139	1819	3,197	94	1 to 36
1761	521	9	1 to 58	1820	2,458	70	1 to 35
1763	488	9	1 to 54	1825	2,740	26	1 to 105
1764	538	12	1 to 49	1826	2,440	81	1 to 30
1766	581	3	1 to 193	1833	2,138	12	1 to 178
1767	664	11	1 to 60	1834	2,024	34	1 to 60
1768	655	9	1 to 41	1835	1,902	34	1 to 56
1772	704	4	1 to 176	1836	1,810	36	1 to 50
1773	694	13	1 to 54	1838	2,126	45	1 to 47
1774	681	21	1 to 32	1844	2,176	14	1 to 155
1782	990	6	1 to 165	1845	1,411	35	1 to 40
1783	1,167	15	1 to 78	1846	2,025	17	1 to 119
1787	1,374	10	1 to 134	1847	1,703	47	1 to 40
1788	1,469	23	1 to 64	1848	1,816	35	1 to 52
1789	1,435	25	1 to 57	1849	2,063	38	1 to 54
1790	1,546	12	1 to 129	1853	1,901	17	1 to 111
1791	1,602	25	1 to 64	1854	1,943	37	1 to 52
1792	1,683	10	1 to 163	1855	1,060	35	1 to 30
1793	1,757	19	1 to 81	1856	1,600	23	1 to 64
1800	1,837	18	1 to 102	1857	1,509	33	1 to 46
1801	1,725	30	1 to 57	1858	1,086	30	1 to 36
1803	2,028	44	1 to 46	1860	1,404	26	1 to 54
1811	2,561	24	1 to 107	1861	1,135	59	1 to 19
1812	2,766	43	1 to 64	1862	800	58	1 to 14
1813	2,484	62	1 to 40	1863	1,228	32	1 to 38
1817	3,473	32	1 to 108	1864	1,184	26	1 to 45½
1818	3,539	56	1 to 64				

ART. IX.—*Clinical Reports and Observations on Medical Cases.*

By J. T. BANKS, M.D.; King's Professor of Physic; Physician to Sir Patrick Dun's Hospital, &c., &c.

SOME cases of surpassing interest have lately come under my observation—cases unlike in many respects to any which have hitherto been observed by me; and, judging from my own experience that they must be of extreme rarity, I trust a brief notice of them may be acceptable to the profession.

The first case I shall narrate is one of gangrene of the lungs, an accurate history of which previous to the patient's admission into Sir Patrick Dun's Hospital, I regret to say, I was unable to obtain, for, owing to extreme debility and breathlessness, he could scarcely speak, and little could be learned from his friends who brought him to the hospital. Mr. Collins, the resident medical scholar, obtained all the information he could on receiving him into the house on the 3rd of December last. He learned that the man was aged thirty-four, and had been from early youth a person of extremely intemperate habits, and also that from the time he had measles, when six years old, he had a "delicate chest." Three months before the date of his admission into hospital he had some pulmonary affection which, from the account given of it, was probably of an acute character. During this illness the expectoration was said to have been very abundant, and to have contained some blood, but not in any considerable quantity. At no period of his life had he hemorrhage from the lungs.

About two months before he sought medical assistance he is said to have spat up a very large quantity of purulent matter, of so offensive and overpowering a nature that the smell had such an effect on himself as to produce nausea and vomiting.

A sudden aggravation of the symptoms under which he had for some time laboured induced him to seek aid, but he was, from the report of Mr. Collins, in a state which indicated speedy dissolution when he was admitted. The prostration was extreme, the expression of his countenance was indicative of terror and anxiety. His respiration was 72 in a minute; his pulse feeble, flickering, and intermitting—on first examination only 82, but soon rising to 130 in a minute. His lips were livid, and the face of death-like paleness; the eyes sunken, and the features pinched. He was harassed by a paroxysmal cough, and by the expectoration

of such an enormous quantity of fluid that it appeared as if he must be speedily suffocated. When I saw him for the first time, a few hours after he came into hospital, I learned these particulars, and on investigation of the case, fully concurred in the opinion formed by Mr. Collins, that the case was one of gangrene of the lung.

Obscure as the disease so often is, there was in this case a combination of symptoms which enabled us unhesitatingly to arrive at the diagnosis. On entering the spacious ward in which the patient lay, the windows of which were open, I was assailed by an odour so insufferably offensive that I fancy my nose would well nigh suffice to enable me to tell what the disease was, being, unfortunately, endowed with a more than ordinarily acute sense of smell. A near approach to the bed was particularly unpleasant, as the atmosphere about him was loaded with the effluvia from the breath, and from the expectoration, which was being brought up in such quantity that an ordinary spitting cup did not answer for its reception—a large vessel being necessary. The expectoration was of a greenish hue, purulent, and of a peculiarly disgusting odour. The patient was intolerant of the slightest movement, believing that any change of position would cause an increase of the expectoration, and thus inevitably deprive him of the power of breathing. An examination of the chest with any degree of accuracy appeared to us, under the circumstances, to be unwarrantable, and in truth, a close and protracted physical examination would be to most people a most painful proceeding. From the moment that the patient was first seen the lethal nature of the disease was manifest, and with that prescience which we so often observe among the dying, he was himself fully persuaded that his end was rapidly approaching.

Stimulants were unsparingly administered; large doses of quinine with nitro-muriatic acid were given, and turpentine inhalations were employed. The stimulants had no influence in checking the downward course of the disease, and after a distressing struggle he died in about forty-eight hours after his admission, having been unconscious for a few hours before his death.

The autopsy revealed the following appearances:—Both lungs were found adherent to the costal pleura, and were of a greenish-black colour; patches of a deeper hue were observed on the surface, which, on being cut into, gave exit to a purilaginous ichor of a most offensive odour. On cutting into the lungs they were found

to be soft and friable, chiefly a dark-green shreddy pulp, from which fluid and air could be pressed. In many parts small yellowish cheesy masses existed. A large anfractuous cavity occupied part of the middle and upper lobes of the right lung. There was no part of the pulmonary tissue which retained a trace of its original texture—part was of a green colour, of some consistence, and part was in a state of liquefaction.

The bronchial membrane was of a deep purple hue, almost black. The trachea, at its bifurcation, was surrounded by a large number of glands of unusual size, as were also the primary bronchi. The pulmonary artery and its branches, as well as the other vessels of the lungs, were traced with much care into the gangrenous mass without the existence of embolism being discovered. Not only was there no healthy lung remaining, but there was no portion solid which could indicate a pre-existing stage of inflammation. In point of fact, the whole extent of both lungs was in a state of sphacelus.

Before the time of Laennec gangrene of the lung was believed to be a not uncommon disease; but he showed how fallacious was this belief. The infrequency of the disease is proved by the fact that many physicians have never met with it in the course of extensive practice. Sir John Forbes says:—"Gangrene of the lungs is an extremely rare disease. I do not think that I ever met with a case in practice, and certainly never witnessed the lesion in the dead body."

Of late years, if we accept the numerous cases of so-called gangrene of the lung which have been published as veritable and unequivocal examples of the disease, we must admit that the opinion of Laennec cannot be received without qualification. I confess to being sceptical as to many of the cases, which terminated in recovery, being gangrene, and I think a consideration of the lesions of the lung, which sometimes closely simulate the disease in question, will fairly lead to this conclusion.

There is no pulmonic disease so likely to be mistaken for gangrene of the lung as *fetid abscess*; and to this point Sir Henry Marsh has called attention in his highly practical and interesting lectures on the subject. He observes:—"When the fœtor is intense, cases of true fetid abscess are described and named as gangrene, or sloughing abscess of the lung, and one cannot be surprised at the mistake, so offensive in some cases is the odour. True gangrene of the lung I believe to be a comparatively rare affection." It

strikes me that much of the error which prevails may be ascribed to the belief that gangrene is one of the terminations of ordinary inflammation of the lung, and when pneumonia terminates in abscess, and that fetid matter is discharged, the conclusion is hastily arrived at that a disease of a much more formidable nature had existed, and the recovery is looked upon as a most unusual event. It must, I think, be admitted that gangrene only occurs as a termination of pneumonia under peculiar conditions of the system, as in malignant forms of the eruptive fevers, dysentery, yellow fever, plague, glanders, &c., &c. In some reported cases of gangrene of the lungs, said to be a sequel of pneumonia, it is highly probable that the gangrene was the original disease, and the inflammation of the neighbouring parts a secondary lesion. Laennec's view of the nature of gangrene of the lung appears to me to be the most reasonable. He says:—"La gangrène du poudmon semble même, le plus souvent, se rapprocher de la nature des affections essentiellement gangreneuses, telles que l'anthrax, la pustule maligne, le charbon pestilentiel, &c., &c. ; et comme dans ces affections, l'inflammation développée autour de la partie gangranée paraît être l'effet, plutôt que la cause de la mortification."

Much interest attaches to the etiology of gangrene of the lung, and much obscurity prevails. There is one class in which gangrene of the lung is said to be very frequent, namely, the insane. This statement is repeated and perpetuated in every book in which the subject of gangrene of the lung is mentioned. Fischel, of Prague, gives the relative frequency of the disease in the insane and the sane. In 1,000 autopsies of sane persons gangrene of the lung was found in 16 bodies, whilst in the insane in 74.

In the hospital for the insane at Ghent, Guislain observed fifteen cases, and he attributes the disease in many instances to obstinate refusal to take food, and the consequent alteration of the blood. In connexion with this observation, I may refer to the case reported by Ribbentrop of a sane person who, after having for some time laboured under cancer of the œsophagus, was attacked with gangrene of the lung; and here the disease was supposed to be the consequence of inanition; but the ingesta may have passed into the bronchi, and thus given rise to the disease. With respect to the great comparative frequency of gangrene of the lung amongst the insane, my experience does not support the statements made by foreign physicians.

I have seen cases of fetid abscess of the lung in the insane, but

I have never seen a case of true gangrene. My friend Dr. Lalor, the Medical Superintendent of the Richmond Asylum, who has been engaged for twenty years in the treatment of the insane, informs me that he has never seen a case, in nearly 3,000, which have passed under his observation.

Of the specimens of gangrene of the lung brought before the Pathological Society (about twelve in thirty years) only one was taken from the body of a lunatic, who had been under the care of Professor Law in the Dundrum Asylum. It seems, at first sight, difficult to reconcile these conflicting statements, but it is after all an easy matter.

The truthfulness of the reports of the Continental physicians I have referred to is beyond all question; we must therefore admit the fact, and seek for an explanation, and I have no doubt that the absence of gangrene in our asylums depends on the improved mode of managing the insane of late years. Formerly all the conditions most likely to favour the development of gangrene and other cognate diseases were in full operation in hospitals for the insane. The improvement is not confined to English hospitals, but has also taken place in Continental hospitals; and doubtless the great relative number of cases of gangrene of the lungs no longer exists. Wunderlich tells us that he learned from an eminent alienist physician that in his establishment, in which the practice prevailed of keeping the patients the whole day in the open air, gangrene of the lung was unknown. Cruveilhier has particularly noticed that gangrene of the lung is frequent among epileptics; and again I must observe that this is not in accordance with my own experience.

The prolonged and excessive use of spirituous liquors, combined with the usual consequences, misery and want, have been supposed to play an important part in the etiology of gangrene of the lung.

In this case lately under my observation, as well as in those reported by Drs. Stokes and Gerhard, the subjects of the disease were confirmed drunkards, but how little does the frequency of gangrene of the lung accord with the presumed causes.

So far from some of the persons who have laboured under the disease being broken down in constitution by intemperance and want, it is stated, particularly by Mosing, that in the epidemic which occurred in the house of correction at Lemberg, those who were attacked were chiefly young and robust individuals.

It is probable that the man whose case has suggested these

observations was long the subject of tuberculosis of the lung; that the lining membrane of the tubercular cavities became the seat of sphacelus, which subsequently extended to the tissue of the lung, and finally, in the last few weeks of his existence, involved both lungs.

The case is in one respect noteworthy, inasmuch as I know of no recorded example of the disease in which the lungs throughout their whole extent were found in a state of putrefaction.

Cerebro-spinal Arachnitis.—Some remarkable and unusual forms of disease have been observed within the last twelve months in this city. Cases of cerebro-spinal disease, or at least presenting phenomena closely resembling the disease with which we were at one time familiar in an epidemic form, have occurred. In the greater number there has been no examination of the body after death; but, however, from a careful consideration of the phenomena observed in them, and a comparison with those cases in which an autopsy has verified the diagnosis, I believe we may assume that the disease, although as regards the discolouration of the skin, so to speak, a new disease, was nevertheless cerebro-spinal arachnitis.

In *The Dublin Medical Press*, May 30, 1866, I published a case of this terrible disease, and I then called attention to the state of the skin, which was of a deep cyanotic hue, and which to the same extent I had never before seen in cerebro-spinal arachnitis.

The subject of the disease was a boy aged fourteen; the duration of his illness was three days. The *post mortem* examination exhibited the ordinary appearances of cerebro-spinal arachnitis. On the 30th of December last I was requested by my friend Dr. Newland to see a patient with him, who had been suddenly taken ill the night before. The history of the case is shortly as follows:—A boy, aged fifteen, a grocer's apprentice, had been, up to the night of the 29th December, apparently in perfect health. He had been three months residing in Dublin, having previously lived in the country. He had exhibited no sign of delicacy—he was extremely well fed and cared for in every respect, and there was nothing in his manner of life, so far as could be ascertained, calculated to act injuriously on his constitution. Late in the evening he was observed to look ill, and he was recommended to go to bed.

On being visited soon after he retired, he complained much of his head and of sickness of his stomach; he had got out of bed, and was so faint as to be unable to get back. At midnight he was

visited by Dr. Newland, who found him complaining of headache, intense thirst, general uneasiness, and sickness of stomach. The next morning a remarkable change in the symptoms had taken place. The legs and arms were thickly covered with spots. The pulse, which the night before was very rapid, was now almost imperceptible; the expression of the face was much altered, he had incessant vomiting, and the restlessness was extreme. About noon I saw him, in consultation with Dr. Newland, who detailed to me the history of the case as I have given it. We found him in a state of great depression. Frequent vomiting, particularly on taking any fluid. The pupils neither dilated nor contracted, but little influenced by light, and there was strabismus of one eye. The legs and arms presented an extraordinary appearance, being covered with spots of a purple colour, of different sizes, some regular and round, and about the size of a florin, others irregular in shape. On passing the finger over the surface, the spots were found to be slightly elevated. The body had some spots, but not many. There was no retraction of the neck, but the boy had complained of pain along the spine, and in the legs, and the sensibility of the surface was exalted. He was pulseless, and the temperature of the extremities was very low.

Again I saw the boy at ten o'clock at night, with Dr. Newland, who kindly permitted my colleague, Dr. Bennett, to see the patient, as he had observed the case in Sir P. Dun's, which had manifested symptoms so closely resembling those in this case. There had been from the first no effort at rally. Stimulants had no effect, even when retained, but generally the brandy was rejected. The first sound of the heart was inaudible, and no pulsation existed in the carotid or femoral arteries. He seemed utterly unconscious of everything. From four o'clock in the afternoon until his death, which occurred in about thirty hours from the onset of the disease, the boy had three attacks of convulsions. The rapidity of this case was greater than that of the case recorded by me in *The Dublin Medical Press*, but the resemblance to it was very striking, and was immediately recognized by Dr. Bennett; and we agreed as to the extreme probability of finding, in the event of our obtaining permission to examine the body, similar appearances in the brain and spinal marrow. It was impossible to obtain a *post mortem* examination.

The case was a fearfully appalling one in the suddenness of the seizure and rapidity with which collapse set in, a few hours only

intervening between the period when he seemed in perfect health and when death was inevitable.

All remedial agents in such a case are alike powerless, nothing seems to have the slightest influence in staying the fatal progress of the disease. This case and the one referred to which, resembled it, were unlike the sporadic cases of cerebro-spinal arachnitis, but forcibly reminded me of the epidemic form with the epiphenomenon of discolouration of the skin and spots. A remarkable and striking feature which had never, I believe, been seen in the cases in France or in this country when the disease was present in an epidemic form, nor until lately in any of the sporadic cases which have come under my own observation since the year 1846.

ART. X.—*Reports in Colonial Practice.* By HUGH CROSKERY, L.R.C.S.I.; Member of the Surgical and of the Obstetrical Societies of Ireland; Fellow of the Obstetrical Society of London; Corresponding Member of the Scientific Association of Trinidad; and late Editor of *The West India Quarterly Magazine*, Chapelton, Jamaica.

1. ON THE VALUE OF TARTAR EMETIC IN COMPRESSION OF THE BRAIN, AND IN CONTROLLING CONVULSIONS AND MANIACAL EXCITEMENTS DEPENDENT THEREON.
2. ON A CASE OF LEUCOCYTHEMIA, COMPLICATED WITH RECURRENT ATTACKS OF HEMATEMESIS AND ASCITES; ALL DEPENDENT ON AN ENORMOUSLY ENLARGED AND INDURATED CONDITION OF THE SPLEEN; WITH REMARKS ON THE SPLEEN AND ITS FUNCTIONS, FOUNDED ON OBSERVATIONS MADE DURING A CLOSE ATTENDANCE ON THIS CASE FOR A PERIOD OF NINE YEARS.
3. CASE OF SPINAL MENINGITIS, TREATED BY OFTEN-REPEATED DOSES OF OPIUM AND QUININE, AIDED BY THE CONTINUOUS APPLICATION OF POWDERED OPIUM, COMBINED WITH MERCURIAL OINTMENT TO A RAW, BLISTERED, SURFACE ALONG THE SPINE; RECOVERY.
4. CASE OF DOUBLE HEPATIC ABSCESS, OCCURRING AS THE RESULT OF A PROLONGED ATTACK OF DYSENTERY; THE FLUID EVACUATED THROUGH PUNCTURES MADE IN THE ABDOMINAL WALL AFTER UNION HAD BEEN ARTIFICIALLY ESTABLISHED BETWEEN THE LATTER AND THE LIVER; RECOVERY.

5. CASE OF EMPYEMA, THE RESULT OF A SUBSCAPULAR ABSCESS HAVING BURST INTO THE CAVITY OF THE CHEST; PUNCTURE THROUGH THE WALLS OF THE ABSCESS; RECOVERY.
6. AN ACCOUNT OF FOUR CASES OF STRANGULATED HERNIA, EACH OF WHICH PRESENTED SOME POINT OF INTEREST.

1.—*On the Value of Tartar Emetic in Compression of the Brain, and in Controlling Convulsions and Maniacal Excitement Dependent Thereon.*

G. H., a young white Creole^a gentleman, had been suffering for some days from languor and pain in the right hypochondrium. On Thursday morning, the 31st of October, 1861, he left his bedroom, about six o'clock, in his usual good health and spirits, and partook of the accustomed Jamaica dish—a cup of good coffee—usually served at that hour of the morning. He went out for a stroll, and was seen to walk with a steady and buoyant gait. An hour afterwards he was found, about five hundred yards from the house, in a fainting state, and with his body lying on the pathway leading through a pasture on the hill-side. Being in the house at the time, I was called to him at once, and found him with contracted pupils, a feeble pulse, a cold, pale, skin, and completely unconscious. He was lying where, a short time before, he had been discovered, with his body up and down the hill, and in such a position as—if coincident circumstances did not point to a different conclusion—to lead to the belief that he had either laid himself down to rest there, or that he had felt faint, and had fallen down at the spot where he had been found: his head rested on a place by no means hard, and his hat was found not far from his person.

When I first saw him, he was in a state of syncope, which might have been the result of a heavy fall, and consequent concussion of the brain, or of sudden faintness from the exertion made use of in ascending or descending the hill. His state of health for some days previous—and the fact that no cuts or bruises about the head, or stones about the spot, could be discovered—tended to convince me of the probability of the latter being the cause of the mishap. With this belief he was kept a short time where he was, in the horizontal posture, and a stimulating salt was applied to the nostrils. In a very short time, however, it became quite evident that he had

^a “Creole” means, simply, born in the West Indies.

suffered, in the first instance, a severe concussion of the brain, and that some rapid effusion was taking place as a consequence; the symptoms of concussion being rapidly succeeded by those of compression. (It was discovered afterwards that, in running down the steep descent, the heel of his boot had slipped on the root of an orange tree which projected out of, and ran for some distance along, the ground, and that he had been, in consequence, precipitated down the hill—his head falling backwards, heavily, on the ground). Unconsciousness soon merged into complete insensibility; the pupils became widely dilated; the face and neck suffused and red; and the body violently convulsed. I had him removed into the house at once, and, before many minutes had elapsed, twenty ounces of dark blood had been taken rapidly, *cum pleno rivo*, from the arm, and a bolus, of fifteen grains of calomel, had been washed down his throat. The convulsions, however, continued to be frequent and violent; and, before another hour had passed, twenty ounces of blood, as dark as before, had, for the second time, been abstracted from the arm.

About nine o'clock, a.m., the convulsions had ceased, but a deep coma remained; the breathing continued to be stertorous; the pupils dilated; and the muscles, principally of flexion, rigid and contracted. A cold evaporating lotion had, from the first, been applied to the head; and evaporation was still encouraged and aided by the steady action of a palmetto fan.

An enema, containing olive oil, turpentine, and a few drops of croton oil suspended in thin mucilage, was given about ten o'clock, and this had the effect of removing, about two hours afterwards, a large quantity of very offensive matter. The pupils continued to be widely dilated; at times the heart would beat violently, and the eyes became suffused; but these bad symptoms were always kept in check by the timely and judicious exhibition of small doses of tartar emetic, repeated as often as the pulse indicated an increased circulation; the hair had, of course, been removed.

This young man remained completely insensible for four days; and it was not until the evening of the fourth that he became at all conscious.

I cannot speak too highly of the value of tartar emetic, and of its powers in such cases as an antiphlogistic and sedative. I desire that this case should be placed on record to prove this; and also because it exhibited many points of interest, which, although they can now be only cursorily referred to, might have been dwelt on at

the time, when all was fresh in the memory, with profit. In Jamaica—and more especially in the country parts, where one has so much saddle-work, and so many wearisome rides through mountain passes, and under such a burning sun—it is impossible to sit down and write after the fatigue of the day is over; much of great interest to the profession might be recorded otherwise; and one sometimes regrets that he has neither the time nor the inclination to place on record what he sees and observes.

I have thus diverged a little from the subject, in order to explain why I am compelled to trust, principally, to memory for the details in this case. But to return to the facts:—I found that small doses of tartar emetic, combined with henbane and digitalis, given frequently, calmed the pulse and warded off many paroxysms of convulsive excitement—so to speak. Of course other items of treatment, viz.: a blister early applied to the back of the neck and head; mercurial dressings to the blistered surface, and mercury and opium given so as to touch the gums; all combined to check and ward off inflammation, and to effect a permanent cure. I wish, however, to speak particularly of the value of tartar emetic in this case, as I found it, and as it impressed itself on my memory at the time. Each apparently approaching fit, as indicated by redness of the face and conjunctiva, fulness of pulse, and jumping carotids, with muscular twitchings, &c., was immediately subdued by the tartar emetic, combined, as it was, with henbane and digitalis.

Strange to say, in this case, when consciousness returned, after a lapse of four days, this young man was a maniac; he had all sorts of fancies, and was at times very much excited; when paper was given to him for cleansing purposes (I will be understood when I so express myself), he would fancy, and insist, that it was all stuck over with pins, and he would pull out the imaginary pins and count them into a hand, or on the table, up to any number; he had lost the memory of names, but not of numbers; and he would wander about the house, and climb up and examine the top of the wardrobe, which his excited imagination would picture as something else. This maniacal excitement, too, was kept in check—and completely so—by tartar emetic, frequently administered; and, from the good effects thus, at all times, made apparent, I was able to assure the boy's sorrowing friends that this excitement would pass over, and that reason would be left intact, after the lapse of a few days; and so it was.

Five years have since elapsed, and my patient of that time is now a thriving planter, and a clever young man.

2.—*On a Case of Leucocythemia, Complicated with Recurrent Attacks of Hematemesis and Ascites; all Dependent on an Enormously Enlarged and Indurated Condition of the Spleen; with Remarks on the Spleen and its Functions, Founded on Observations made during a close attendance on this Case for a period of Nine Years.*

I published a report of this case in *The Dublin Hospital Gazette* of the 1st of February, 1858; and, again, a further report in the same Journal of the 1st of August, in the same year.

It becomes my duty again, for the third time, to bring this case under the notice of the profession; and I make no apology for so doing, because I have had further opportunities of observation, and because, since the publication of my former reports, much has occurred, during the progress of the case, greatly to interest the medical observer.

I regret, however, that the demands of an extensive and wearisome practice has not permitted me to take notes of interesting details, from time to time, and that I shall only be able to give, from memory, a synopsis of symptoms, and of the mode of treatment employed. I quote, however, from notes of that date:—"In September, 1857, Master Edward Ewbank had an enlarged spleen, from repeated attacks of intermittent fever, and residence in a malarious district. I was suddenly sent for; and, on my arrival, I found that the child, who was only three years of age, had vomited about twelve ounces of blood. There was a good deal of excitement of the system, as indicated by carotid pulsation, and a small and rapid pulse at the wrist. Small doses of turpentine, beaten up with yolk of egg, were given, at intervals of ten minutes, with success.

"On the following day the hemorrhage again returned; the same treatment being adopted. He was kept quiet, and aloof from all excitement, for several days after this attack; and a mixture of sulphuric acid and digitalis, with infusion of rose leaves, was administered, at intervals of three or four hours, throughout the day.

"Hematemesis, however, returned ten days after the last attack, and was, on this occasion, accompanied by melena. For the turpentine small doses of sugar of lead were now substituted, but with little better effect than that of arresting the hemorrhage for the time. When the stomach became irritable, the lead was given by enema.

"On Saturday night, the 12th of September, he not only had a return of the hematemesis, blood being lost in large quantities, but he coughed up blood also from the lungs. I should mention that, from the first, the spleen was of an enormous size, and that it occupied, in the first instance, nearly one half of the child's abdominal cavity. From repeated blistering, and the application of iodine paint, it had now become considerably reduced in size.

"I was summoned, early on the morning of the 13th; and, on my arrival at the house, I found the child cold, convulsed, almost pulseless, and with pupils dilated almost to their entire extent.

"The first strong stimulant that came to hand, viz., camphorated rum—which is always to be found in Jamaica homes—was at once thrown up, in considerable quantity, by enema, for the child could not swallow, and was apparently dead; and the body was wrapped up in warm flannels, sinapisms being at the same time applied. The enema was repeated, and with the most happy results. Within an hour and a-half after I had entered the house the pulse had got up, its natural heat had returned to the body, and the child, which had been in a state of syncope for four hours, became conscious."

It was quite evident to me that the spleen was the primary cause of all this mischief, and that, if this boy's life was to be saved, I must adopt some course by which an immediate reduction of the size of this organ should be effected; while, at the same time, the stomach should be kept in a perfectly quiescent state.

I shall not, at this stage of the paper, enter further into the subject of the functions of the spleen, than to state my reasons for adopting the course to be presently described, and which I found to be so effectual.

The spleen is so intimately connected with the stomach by vessels, and otherwise, that it is easily understood how congestion of the one is associated with congestion of the other, and *vice versa*. Here I had to deal with a case of enormously enlarged and indurated spleen, with recurrent hemorrhage as the immediate consequence of any sudden increase of its volume. It was, then, not difficult to come to the conclusion that each time at which blood was shed was merely an over-effort of nature to rid the vessels of the great end of the stomach—where so intimate an anastomotic circulation is kept up between them and the sinistra branch of the splenic—of the unusual quantity of blood circulating through them.

I resolved, then, in order to give the stomach perfect rest, to

sustain life by enemata alone, and not to give a single morsel of anything by the mouth for some time. Accordingly, the child was kept alive for the next ten days entirely by food injected into the rectum. Enemata of rich chicken broth or beef-tea, when beef could be obtained, were administered every second hour, night and day, and were almost all retained and absorbed.

At the same time, in order to improve the quality of the blood, and to effect a reduction of the size of the splenic tumour, two grains of sulphate of quinine, and two of sulphate of iron, with sufficient sulphuric acid to dissolve them, were added to, and given with, each second enema, or even more frequently at first.

Besides this combination of quinine and iron by enema, a dose of ten drops of the syrup of the iodide of iron, in a small quantity of water, was given three times a day—this being the only fluid thus taken—by the mouth; while iodine paint, or ointment, was duly applied over the region of the spleen.

Under this treatment the hemorrhage from the stomach was completely arrested, and did not recur again for some years, and the child gradually gained strength. After ten days the enemata were entirely given up, and nourishment and his quinine and iron were given by the mouth.

At this time, however, the spleen, although much smaller, was still bulky enough to cause irritation and mischief. My notes of that time somewhat thus describe it:—

“The spleen is still enlarged, and can be felt in its entire outline. It occupies the left hypochondriac and lumbar regions, and projects into the umbilical, where a deep notch can be felt at the hylus, which projects forwards, instead of looking backwards and inwards, as in the natural state. The tumour can be moved backwards and forwards with the hand.”

No sooner had the hemorrhage been conquered, than one of the other sequæ of enlarged spleen appeared. Ascites suddenly set in. As soon as I found this to be the case, I resolved to have the child moved, at all hazards, from the bed of malaria, where his father's house was situate, to a more healthy and elevated spot. This was at once done; and although vapour baths, diuretics, and diaphoretics, &c., were given, with the view to bring into increased action the kidneys and skin—and, at the same time, with the aid of the purgative action of compound powdered jalap, to remove the fluid that had become so rapidly secreted—the abdominal cavity was so much distended on the fourth day as to interfere materially with

respiration. In addition, the poor little fellow, who, as I said before, was only three years of age, had incessant thirst, and a dry harassing cough.

Still the urine continued scanty, high coloured, and of high specific gravity; and the skin remained rough and dry.

I had been giving him, up to this period, the 30th of September (symptoms of ascites having been observed, for the first time, on the 26th), a mixture of the solution of acetate of ammonia, with acetate of potass, a small proportion of tartar emetic, and sweet spirits of nitre, in camphor julep; but, as the disease had progressed so rapidly, and the above combination appeared to do no good, I omitted it, and substituted for it a simple mixture of cream of tartar, sugar and water, with a proportion of spirits of nitric ether in it—the child to drink about an ounce of this every hour. The result was surprising. In about six hours a decided change took place, and the kidneys began to act powerfully. The next morning the circumference of the belly was one inch and a half less than it had been the day previous, and he had copious alvine evacuations during the night. The mixture was, of course, continued, and the child allowed to drink it *ad libitum*. I should mention that I had never omitted to look after the condition of the spleen, and that the child continued to take his iron as before.

On the fourth day after the cream of tartar had been commenced the girth of the abdomen had been reduced by four inches; and its walls, which had heretofore been stretched even to bursting, were now loose and flaccid. His vapour baths at night were continued, and a few drops of paregoric and of compound spirit of ether added to his mixture, completely relieved his cough. His skin also was soft and moist, and the kidneys were acting profusely.

But all the good that had been done was neutralized by carelessness on the part of his nurse, who allowed him to lie naked in his bed after he had a vapour bath—the weather at the time being moist and unfavourable. The result of this carelessness was a rapid re-accumulation of fluid, notwithstanding all the means taken to prevent it.

The child was tapped, and ninety-two ounces of a clear, straw-coloured fluid, highly albuminoid, and of a specific gravity of 1010, removed. He was tightly swathed in flannel; and a mixture given, containing bitartrate of potash, nitric ether, acetate of ammonia, tincture of digitalis, and tartar emetic. Besides this mixture, a dose of ten grains of saccharated carbonate of iron was given, three

times a day, and the bowels were kept freely open by occasional doses of compound powder of jalap.

Notwithstanding all this, the abdominal cavity filled up rapidly for the second time; and the feet, legs, and scrotum became œdematous. His breathing having become much oppressed, he had to be tapped again on the twenty-fifth of October, the first tapping having taken place thirteen days previously. On this occasion a large wash-hand basin of fluid was drawn off.

Again was the prescription altered; and a combination of nitrate and bitartrate of potash and sweet spirits of nitre, made up into a mixture with syrup and water, was given instead, in tablespoonful doses, every third hour.

He was also ordered to take citrate of iron and quinine, with syrup of iodide of iron.

To further stimulate the absorbents an even and regulated pressure was kept up over the abdomen.

Six days after the second tapping the abdomen had again become full of fluid, notwithstanding all efforts made, in the meantime, to restore the balance between secretion and absorption; and, besides the swellings in the dependent parts of his body, his face began to be œdematous.

As a last resource, I determined to give mercury; and gave three grains of hydrargyrum cum cretâ, and two of antimonial powder, every third hour. I discontinued all else except a diuretic mixture.

The mercury was commenced on the morning of the 1st of November; and on the evening of the 2nd its good effects had become plainly evident. His appetite, heretofore inordinate and depraved, now became regular; the swellings in the legs, face, and scrotum had already disappeared; the circumference of the abdomen was, by measurement, ascertained to be less; the kidneys were acting freely, and a sensible perspiration pervaded the entire surface of the body.

Ten days after the mercurial treatment had been commenced every symptom of the disease had disappeared; and the child had gained strength to such a degree as to be able to run about the house.

Under the influence of iron and quinine, administered for some time after, and at such periods as the spleen showed an increase in size, from malarious influences, or occasional attacks of intermittent fever, this boy became, comparatively speaking, well; and he had,

for some years, an exemption from any attacks of hemorrhage, or from any return of secretion of fluid in the cavity of the belly.

When he became a lad of eleven, and while at a boarding school, in a rather humid locality, in Jamaica, he got fever, with of course, an increase in size of the splenic tumour. A severe attack of hematemesis again occurred, and recurred as before; but, under treatment he again became comparatively well.

He never, however, lost the waxy, chlorotic, leucocythemic appearance which I have always observed to be peculiar to people who suffer from splenic affections. Up to this period, and ever since he had been three years old, he always carried in his countenance the marks of his hidden affection; and, although he became a smart, lively lad, and could gallop his pony about the country like other boys of his age, his waxy look, which never left him, indicated, to the medical observer, a bad prognosis in regard to the future.

I have always made the remark, that this lad would, again and again, be subject to attacks of vomiting of blood; and I have, over and over again, tried to impress on his parents the absolute necessity of their entering into some arrangement by which he could be sent to a colder climate. I am strongly of opinion that, had my advice been taken, the lad's spleen would have, by the natural effects of climate alone, become so much reduced in size as to be enabled again to perform its proper functions in the animal economy; and I should not now have the painful duty imposed upon me of relating how accurately the unfavourable opinion which I formed of this case in the past has been verified.

On two occasions, within a few months of each other, and after the boy had attained the age of twelve years, he had attacks of hematemesis. On the first occasion, bright red blood came up; on the second, he vomited blood of a darker hue, and passed melanotic stools afterwards.

In both attacks I was quickly with him. Full doses of sulphuric acid was the treatment generally employed, in the first instance, with a blister or strong tincture of iodine applied to the spleen. After twelve hours, or so, according to circumstances, I poured in, frequently, small doses of sulphate of iron and sulphate of quinine, with an excess of acid (sulphuric) in each draught. This mode of treatment always checked the hemorrhage, in the first instance; while, at the same time, it tended to improve the quality of the blood, thus telling on the functions of the spleen for the time. Last—not least—it always had the effect of reducing somewhat in

size the enlarged and indurated tumour, which was the cause of all this mischief.

Not long after the last attack of throwing up blood, and notwithstanding the administration, constantly, of iron and quinine tonics—or rather the ordering of them, for I believe they were omitted oftener than they were taken—notwithstanding my often repeated assurances as to the necessity of their never being omitted—the boy got ascites.

On the 2nd of September, 1866, it became very evident that the spleen, which was always movable and in direct apposition with the abdominal wall, was now more easily moved than ever; and that, on being pushed gently away, it displaced fluid on its return to its old position. The usual mode of ascertaining the presence of fluid in the cavity of the abdomen showed also, but too plainly, that the boy was in for ascites for the second time, and after the lapse of upwards of nine years, since the first prolonged attack.

His measurement round the body was, on the second of September, 29 inches over the navel, and $32\frac{1}{2}$ inches higher up, over the greatest convexity of the splenic tumour. I examined him, by measurement, at intervals, in the latter region, in order to be able to ascertain the effect of treatment on the spleen.

Quinine and iron were poured in now, more than ever, as much as the stomach, which was frequently irritable, would admit; the spleen was constantly painted with iodine—or rather the abdominal wall over and around it—diuretics and diaphoretics were given, and vapour baths at night, closed the remedial treatment of the day.

I should mention that, immediately before this last attack of dropsy, his mother had, very unfortunately, given him a dose of salts without my sanction or knowledge; and while I was trying hard to improve the quality of his blood, this draught ended all the good that had previously been effected. I should also mention that, at this particular time, an ulcer on his leg, which had previously been of an indolent character, now assumed a sloughy black appearance, and the odour from it was intolerable; a few poultices, however, made with bread and a weak solution of chloride of lime, or soda, soon put this to rights, and the sloughs were cast off, leaving the sore clean, but open as before.

Between the dates of the 2nd and the 9th of September the measurement of the girth round the body varied, one day being less, and again becoming increased. On the 13th, he had become

considerably enlarged, the girth measuring, as before, now being $32\frac{1}{2}$, $33\frac{1}{2}$.

In addition to the other items of treatment, I now resolved to try, with caution, a few alterative doses of mercury, and he was, accordingly, given one grain of grey powder combined with two grains of Dover's, every third hour. On the 15th instant he was very much reduced in size, and the mercury and Dover's powder were continued. On the 17th I omitted the grey powder, as the size of the abdomen remained stationary, and gave him four grains of Dover's powder, three times a day instead. Instructions were given, at the same time, to keep the bowels open, by giving an occasional dose of compound powder of jalap.

On the 20th, I made the following note:—"I wish him to take more iron in the day, and to begin to take it sooner, so that he may be able to take it, at least, four times in the twenty-four hours. In order to be able to do so, he may take less of the diu-diaphoretic mixture. Continue the Dover's powders as before."

On the 24th, finding the spleen larger, I directed that he should take 30 drops of the syrup of iodide of iron, three times a day, instead of the former preparation of quinine and iron, which, however, I directed to be taken every alternate day, with the syrup.

On the evening of the 24th, I was summoned to him, and found that the sudden enlargement of the spleen, observed in the morning, had, true to former instinct, terminated in another attack of hematemesis. I stopped all other items of treatment, placed a blister over the spleen, and ordered ten drops of elixir of vitriol, to be taken through a tube, every third hour, in two-thirds of a wineglass of water. The girth, over the spleen, was now $35\frac{1}{2}$, while that over the umbilicus was 33 inches.

On the 26th, I ordered that two grains of quinine should be added to each acid draught; and on the 27th, in addition to this, he resumed his iodide of iron.

After this date, the abdominal fluid became secreted with great rapidity; and as I clearly saw that I should have to tap him at no distant date, I continued to pour in as much iron and quinine as could be taken, in order to strengthen him, reduce the spleen, and improve the quality of the blood as much as possible.

I should now mention that, on submitting my opinions on the case, with the mode of treatment employed, by letter, to an eminent professional brother, Doctor Fiddes, of Kingston, that gentleman—who occupies, and deservedly so, in the estimation of the public

and of the profession, the distinguished position of "head" of the surgical profession in Jamaica—did me the honour to approve fully of my views and treatment. He advocated, as the last resource, and as almost the only chance of saving the boy's life, a step which I had often before recommended in vain, viz., the boy's removal to a colder climate, if his strength would admit of it.

On the third of October I tapped him, and removed $10\frac{1}{2}$ quarts fluid. The boy bore the operation, which was performed with great rapidity, well. A flannel bandage, cut into tails at both ends, which were held on each side by assistants, and drawn tighter as the fluid escaped, kept up a steady and even pressure; and before many minutes had elapsed, the abdominal cavity, full before, even to bursting, had emptied itself almost entirely. A little champagne given; the lips of the puncture sealed up close with court plaster; a pillow of soft lint being then placed over all; the bandages drawn down, and carefully and evenly adjusted, and its ends sewn over each other (for a knot would have exerted injurious pressure, and have caused pain). All this being done, the boy fell asleep with a sigh of relief.

Bandaged as he now lay, the enormous size of the spleen was apparent, even to the eye. It projected as an enormous tumour, occupying the greater portion of the left side of the belly, pushing out the bandage before it; while the belly, on the opposite side, was sunken and flat.

The condition of the removed fluid was equally striking. It frothed up into a dense mass of foam, which did not subside. I had no means of ascertaining its specific gravity; but, when I add that, on a portion of it being boiled, it was found to contain almost one-third of solid albumen, it will be seen that its density was high.

I need not enter further into the treatment of the case than to say that the boy was up and about in a couple of days; that he had a good appetite; and that he continued to take his iron and quinine frequently, and a draught of nitre in some gin toddy at bed time. I persuaded his father to remove him, a week after, to the sea-side, where he could have warm sea-baths, and breathe an abundant supply of pure ocean air; while he would have, at the same time, the benefit of the combined skill of the medical men of Kingston.

Before he left my charge—and I saw him, for the last time, five days after he had been tapped—the fluid had again formed in the cavity of his abdomen, to the extent of, at least, four quarts; so

that the prognosis to be formed, in regard to his ultimate recovery, is not a very cheering one.

I shall now proceed to make some general remarks on this case, and

On the Spleen and its Functions.

In the foregoing case there has been always an extreme want of colouring matter in the general mass of the blood. The boy is white and chlorotic looking—like faded wax—and he has been so for the past nine years. Were it possible to examine his blood in the field of the microscope, it would be found to abound in white corpuscles, and to be greatly wanting in solid constituents. This is, in fact, a case of aggravated leucocythemia; and, perhaps, as pure a case of this affection as has ever been seen. During its whole progress, quinine, and the various preparations of iron, given in different forms, as they seemed to be required, and changed when the stomach appeared to prefer one preparation to another—which have usually such an effect in improving the quality and colour of the blood, and enriching it with solid matter, had not, in this case, the full effect generally expected from their use. And why? Because the spleen was powerless and inactive; incapable, in fact, of doing its usual duty, *i.e.*, of maturing red corpuscles, and supplying them to the general mass of the blood.

I have never heard this boy complain of the pain usually felt by chlorotic people, in the region of the spleen; unless, indeed, when the organ was struck sharply by the point of the finger in making an examination. Granting that the function of the spleen is that of maturing red corpuscles, and of supplying them to the general mass of the blood, the organ, in this case, has not done its duty, although it has been aided and stimulated, in every possible way, by blood-enriching tonics frequently administered.

The fact of this child having lived and grown to be a big boy of twelve years of age—all the while looking pallid and waxy, and more especially so when the spleen, from miasmatic or other causes, appeared to become increased in volume; all this, most assuredly, tends to prove that the function of the spleen is essentially that of supplying colouring matter to the blood, and, through it, to the tissues generally.

In this case, the organ, from its enormous size and its extreme hardness and lobular condition, must have lost, almost entirely, and that years ago, its functional use in the animal economy. It

does not supply blood rich in red corpuscles to the system, for the lips are, and have been for years, very pallid, without, apparently, a particle of colouring matter in the blood circulating through them; and the cutaneous surface, generally, has had, for the past nine years, a faded shining appearance.

Any scratch, too, or sore on the child's body would continue in an indolent, unhealthy state; and should granulations at last spring up under the blood-improving powers of iron and quinine, they would be pallid and flabby, showing very little determination to coalesce.

The theory that the spleen acts as a diverticulum for the superfluous blood of the system, and that this is its main use in the economy, receives a severe blow from facts observed in this case. Here we have an enormous, indurated, and lobulated organ, at all times with its capsular covering and processes so much distended, as to be incapable of further sudden distention, or slackening, and, consequently, incapable of receiving or retaining any sudden accession of blood introduced into the circulation, or thrown back upon the spleen itself during any temporary congestion of neighbouring organs and vessels.

The sudden enlargement of the spleen, observed after the process of digestion has advanced as far as the formation of chyle in the duodenum, appears to me to be dependent more on the blood of its own vein being sent back upon the spleen, by pressure of the duodenum and its contents upon it (the vein), than upon any sudden accession of blood to the system at the completion of the process of digestion.

I speak now of the healthy state; but, of course, in congestion of the liver or stomach, or in any arrest of the circulation through the portal vessels, the blood must be thrown back, and is thrown back—from the absence of valves to prevent it—upon the spleen, and then congestion of the latter organ occurs as a matter of consequence.

In this very case the enormous hardened mass into which the organ has been converted, I doubt not, maintains and enlarges its own proportions, and keeps itself full always to distention, by exerting, *per se*, continuous pressure on the vena portæ, thus driving back the blood directly on the spleen, through the splenic; while, at the same time, it keeps up a steady distention of the mesenteric veins; and, as a natural consequence, causes a constant inpouring of sero-albuminoid fluid from these vessels into the

abdominal cavity. As an additional proof of the truth of the foregoing statement, I would add, that for some time past, the surface of the belly has been covered with an extensive network of distended veins, which ramify in every direction, and clearly show by their numbers and distended condition, an obstruction to the flow of blood upwards through the portal vessels. This I consider to be the pathological explanation of the case, and to account fully for all the symptoms, and I have no doubt but that the profession will fully support me in taking this view of the matter.

In corresponding with Doctor Fiddes, of Kingston, who has now charge of this case, I have proposed that some mechanical apparatus should be made by which the spleen could be lifted from off the vessels behind, and injurious pressure on them thereby prevented. For instance, a bandage could be made, with a firm cup-like cavity, the convexity of which might be made to press on the tumour from behind, and thus to raise the spleen, and push it to the front. This suggestion is, at any rate, worthy of trial; and I make it, leaving to the profession to put it in practice in similar cases, according to circumstances, and to report on its value.

3.—*Case of Spinal Meningitis, Treated by Quinine and Opium, with Mercurial Inunction; Recovery.*

Mrs. E., a married lady, aged fifty, had been complaining, for some days, of a very severe pain in the back, increased, by motion, over the lower dorsal vertebræ. She had, about six months before, when on a visit in another part of the island, suffered from a prior attack, not by any means so severe, according to her husband's report, as the one I am about to describe, of spinal irritation.

Warned by previous experience, she immediately retired to bed, and took measures to clear out the bowels. I believe she took a full dose of calomel, with a grain of opium in it, and that she followed up this, the day after, with some senna-tea. At the same time she applied a good-sized blister, much too large, however, over the seat of pain. The *primæ viæ* were, I believe, well cleared out, but with no relief to the pain in the back, which continued to be very severe, while the stomach was exceedingly irritable.

On the second night after the accession of pain she tossed about the bed, and had an uneasy troubled sleep, complaining, at times, of severe cramps in her legs and thighs, all shooting from the back. Friction was employed, but all to no purpose. At midnight, she cried out, "a fit—a fit!" and immediately she became violently

convulsed over the whole body, her trunk being bent forwards in a state of perfect opisthotonos. These convulsions recurred several times before morning, each fit being more severe than the other preceding it, until she became quite exhausted.

I saw her about seven o'clock, a.m., when the preceding history of symptoms was related to me. I found her in a comatose state, with the pulse small and weak, skin clammy, and frequent subsultus. The tongue appeared to have been a good deal lacerated, although every care was said to have been taken, and had been taken, to prevent it.

There was evidently no time to be lost if a recurrence of strong convulsions was to be prevented. I saw, however, that I had a good foundation to build on, in as much as the bowels had been well cleared out, and I had an open blister on the back for local applications. On looking at the latter, I was horrified that such an extensive surface had been blistered—and such a blister it was! I pause here to state, that I had no doubt but that the extreme irritation caused by this large raw surface materially affected the severity of the attack.

At least four grains of powdered opium were rubbed up with about a drachm—for there was no time to weigh one or other—of mild mercurial ointment; and this was quickly spread and applied to the raw blistered surface. Frictions to the legs, and warmth applied, by mustard cataplasms and blankets, to the legs and thighs, were also enjoined. In the meantime I had weighed and made up some powders, each containing two grains of quinine and one grain of opium. I directed that one of these should be given every hour during the day.

The result of the above treatment was that the muscular twitchings ceased entirely for many hours. In the evening, however, she again became restless—her muscles twitched, and convulsions were imminent. A fresh plaster of opium and mercurial ointment, with even a greater proportion of finely-powdered opium in it than before, was at once applied, and a large warm poultice was placed over all. Frictions again; and warmth applied to the limbs, while cold applications were kept to the head, all combined to ward off the attack.

Still she continued restless, and, although opium had been used so unsparingly during the day—locally applied and otherwise—I now gave her a full dose of thirty drops of Battley's sedative. Her skin then assumed a comfortable warmth, and she slept quietly throughout the night.

On the next morning she was quite calm and collected, and felt no other unpleasant sensation, except that of extreme exhaustion. She said that she felt "miserably weak."

She had not remembered anything that had occurred since her sensations immediately before the first convulsion, when she said she "felt that she was about to have a fit." I now continued to apply a weaker preparation of the opium and mercurial ointment, with moist heat over all, in the shape of a poultice, when she complained of pain in the back or limbs; and the powders of quinine and opium were now given, in the proportion of one every third hour. Still she continued to have, at times, ugly sensations, and a terrible dread of approaching convulsions. Pain in the back was always relieved by moist heat, and an opiate plaster, applied to the still raw blistered surface.

I continued to give a few drops of Battley's sedative at bed-time.

It appeared strange to me that the mercury did not touch the gums, but I was very glad of it on account of the lacerated condition of the mouth. On the third day of my attendance, however, the mercury began to run off by the bowels, and a very troublesome diarrhea set in. I failed to stop this with chalk and catechu, combined with carminatives; and lead and opium, although given frequently, both by the mouth and by enema, had not the slightest effect in arresting this troublesome complication, which now, in reality, became very alarming. Port wine had been frequently given since the occurrence of this debilitating diarrhea; and, in addition to the lead and opium, she continued to take opium with quinine, and to have opium also applied to the sore on the back; still the watery discharges continued.

At this stage of the case I remembered that on a former occasion I had immediately checked a sharp attack of over-purgation by the administration of a single twenty-grain dose of compound powdered kino, and, accordingly, I now prescribed it in this case.

Two doses arrested the discharge, and that within a few hours, and it never returned. The immediate effect was both gratifying and surprising.

I shall not pursue this case further than to state that this lady fully recovered. I have now sent her to the baths at Raetown, near Kingston, for sea bathing, where I have no doubt she will fully recover her health.

I have recorded this case in order to be able to show what great results may be obtained from the free exhibition of quinine, and

opium, and mercury combined, in spinal affections. This lady took enormous doses of opium, repeated, too, at frequent intervals; and the drug was introduced into the system as well by inunction, combined with mercury, over a raw surface, at the most painful part of the spine. And yet, although administered so freely, it did not produce a single bad symptom.

I should recommend in urgent cases the application of the actual cautery to the most painful part of the spine, in order that in addition to the immediate effect produced by cauterization, a raw surface might be at once obtained for the local application of mercury and opium at the very spot where the *fons et origo mali* lie.

In this case I had an extremely raw (but, unfortunately, rather an extensive) surface ready to hand for the application of local remedies; and it will have been observed how the application of finely powdered opium to this sore succeeded in warding off an impending convulsive seizure; and how, at a subsequent period, muscular twitchings and extreme restlessness and anxiety were at once quieted by the same means being adopted.

Had this lady's mouth not been so much cut up and injured I should have combined calomel with opium and quinine; because I believe that the sooner we get the system under the influence of mercury in such cases the better; and that for obvious reasons the prevention of organic change being the most important.

One more remark I would make in reference to this interesting case. The complication of diarrhea, and its persistence, notwithstanding the full and oft-repeated exhibition of opium, surprised me, and its non-debilitating effects, as observed after in the comparatively-speaking speedy convalescence which resulted, surprised me more. Certainly such cases have an extreme tolerance of opium; and does not this fact alone indicate its free exhibition?

In conclusion let me offer that meed of praise to the French physicians, M. Gaussaud and M. Chauffard, to which they are entitled for their writings on this fatal disease; and for their suggestions in regard to the most correct mode of treatment to be employed. Gaussaud, regarding the disease as a kind of spinal fever, treated it with purgatives, and oft-repeated doses of quinine, and he found that under this treatment only two out of 162 soldiers, attacked by the disease, died.

Chauffard found opium, in full doses, given internally, and in some cases combined with quinine, to be most effectual in his practice.

Our own esteemed and lamented Dr. Mayne, of Dublin, during the epidemic of the disease in Ireland, in 1846, found the mercurial plan, aided by blisters and mercurial inunction along the spine, to be the best and most successful mode of treatment.

To the combined treatment, as recommended by all three, I subscribe; strongly urging, at the same time, the immediate and powerful aid to be derived from the continuous application of opium, which may, with benefit, be combined with mercurial ointment, to a raw surface, or raw surfaces, along the spine.

The unpleasant question must now be asked:—Will this lady, at future periods (for it must be remembered that she has had two attacks, at different times, already) be subject to recurrent attacks of this dangerous spinal affection?

I should have mentioned, in a former part of this paper, that Mrs. E. had, prior to her first spinal attack, been a martyr to acute rheumatism; and that she has always been a woman of the most sanguine and excitable temperament.

4.—*Case of Double Hepatic Abscess, occurring as the Result of a Prolonged Attack of Dysentery; the Fluid Evacuated through a Puncture made in the Abdominal Wall, after Union had been Artificially Established between the latter and the Liver; Recovery.*

Several years ago, a Mustee^a man, named Archibald Morrison, aged about forty-five, was under my care for chronic dysentery, from which he had been suffering for some time, and by which he had become reduced in bulk, and miserably weak. He suffered much from a constant dull aching pain in the right side and shoulder; while his yellow skin became still more yellow, and his conjunctivæ discoloured from biliary infiltration.

When he had been under my care for some time, and pain and swelling, in the hepatic region became more urgent, I made a careful examination of his right side, which appeared considerably larger, in the hypochondriac region, than the left, even before his clothes had been removed.

The extensive dulness, on percussion; the deep-seated, dull, dragging pain, and sense of weight in the right hypochondrium; the short breathing, and dry cough; coupled with the fact of his having suffered from a pre-existent and prolonged attack of chronic

^a A Mustee means one born of parents one of whom is a white person and the other a mulatto—the latter being half white and half black.

dysentery—all pointed to the liver as being the diseased organ. It was blistered; mercury was given, but with care, as the man had been previously wasted, from his dysenteric affection; and nitro-muriatic acid, which is my favourite tonic in cases of sluggish liver, and after fevers, was frequently administered.

I soon, however, discovered unmistakable signs of the formation of an abscess. A tumour projected from beneath the ribs in front, of stony hardness in its circumference, but, evidently, softer towards its centre. Rigors, and hectic sweats at night, with a mitigation of the acute pain, and daily increasing fulness beneath the ribs, confirmed the diagnosis; and I became certain that a large abscess had formed.

I will admit that I had very little hope for this poor fellow; but I felt it to be my duty to do all I could to save him. He lived a very short distance from my residence, and I was, therefore, able to see him constantly. I brought his case to the notice of the good rector^a of the parish; and, between us, we supplied him with delicacies, and with a constant supply of good port wine.

I daily painted the circumference of the tumour with a very strong solution of nitrate of silver, having first ascertained that there were no adhesions, and that the abdominal wall could be rubbed and moved freely over the surface of the tumour at all points. Adhesive inflammation set in, after a time, in the ring formed by the application of the caustic, which was, nevertheless, steadily and continuously applied for at least four weeks. The centre of the tumour became, every day, more soft and fluctuating; and the abdominal parietes, at this point, became thinner and thinner. I could not move the abdominal wall here now. It appeared to have become firmly attached to the organ beneath; and the time had come to evacuate the abscess. Accordingly, and not without some degree of anxiety, I punctured the abscess with a long-bladed knife, and immediately a fetid stream issued forth. At least a quart of thin, yellow, horribly fetid stuff came away at the time. The abscess continued to discharge for, I believe, three weeks after; and the swelling gradually subsided. The man continued to be well fed, and to take nitro-muriatic acid three times a day. He perfectly recovered; and I frequently see him, now, working in his cane field; nearly four years having elapsed since the abscess was opened in his side.

^a The Rev. C. H. Hall.

In conclusion I have to add that the dysenteric attack, and the formation of the hepatic abscess in this case, stood, in the relation to each other, as cause and effect. It can be easily imagined how readily a prolonged attack of dysentery, where pus is freely secreted by the mucous membrane of the rectum, may terminate in the formation of one or more abscesses in the liver. The explanation is very simple. The internal hemorrhoidal veins become charged with pus globules, which are carried up, by the portal circulation, to the liver, where they lodge and multiply; and an abscess, or abscesses, more frequently, is the result. Since writing the foregoing, I have this day (Oct. 22nd) seen and examined Archibald Morrison.

I find that there were two distinct abscesses, which I opened, within the ring of adhesion, at different times, a large quantity of pus being discharged on each occasion. The liver appears to have acquired its natural size; and the abdominal wall, in front of it, is considerably puckered and drawn in; showing clearly, and without doubt, complete union between it and the organ beneath. The cicatrices of the two openings, made by the knife, are very apparent, and have an extraordinary appearance. They are situated in the very centre of the body, between the umbilicus and the ensiform cartilage, being about two inches distant from each other; and each having a depression, or pit, in its centre.

The man tells me that he enjoys very good health, with the exception that he sometimes feels an uneasy sensation in the right side, which, indeed, is not to be wondered at.

5.—Case of Empyema, the result of a Subscapular Abscess having Burst into the Cavity of the Chest; Puncture through the Walls of the Abscess; Recovery.

A case of a child, of three years of age, was brought to me some months ago, by a naval medical friend, Dr. Goodman, of Her Majesty's Royal Naval Hospital, Port Royal, who happened to be in my neighbourhood at the time.

This may have been, originally, a case of abscess, with the super-vention of empyema, as the consequence of such abscess having burst into the cavity of the chest. But we had no data to go upon, the persons who brought the child for medical advice having known nothing, or comparatively nothing, of its previous history.

When I saw the child it had an enormous fluctuating tumour in the subscapular space on the right side. This was easily recognized

to be an abscess. During the act of inspiration it became exceedingly firm and prominent, while it became less dense when air was expelled from the chest. This fact did not favour the prognosis, that the abscess was connected with the pleural cavity; for, in such cases, the tumour is, generally speaking, observed to become full during expiration, and to recede during inspiration. Respiration was painful, and the chest, on the affected side, looked considerably fuller than on the left.

Physical signs could not tell us much, for the child yelled and screamed every time we approached it—so much so, that it became impossible to gain any information in this way. It was a perfect little tyrant; and, unless held forcibly down, could not be kept in one position.

An exploring needle, plunged into the midst of the tumour, brought out pus; and, on this opening being enlarged, a stream of fetid matter burst forth, *e pleno rivo*, intermixed with bubbles of air.

At each effort at expiration, the force of the stream became increased, and matter continued to flow until, at least, a pint and a half had been evacuated. A pad of lint was now placed over the opening, and secured there, with the intent that it should act as a valve—by adhesive straps—and a bandage, passed around the chest, kept the parietes of the abscess—which, before this expedient was adopted—flapped, as air rushed in and out of the opening, together.

This child is now in good health, having, strange to relate, recovered rapidly, and without a single bad symptom.

The question now to be asked is—was this, originally, a case of abscess, with the subsequent supervention of empyema, as the consequence of such abscess having burst into the pleural cavity? or was it a case of empyema, in the first instance, with the formation of an abscess in the subscapular region, from the matter having found its way outwardly afterwards?

I am inclined to believe that the former is the true interpretation. It is not uncommon to find nature attempting a cure by the formation of subcutaneous abscesses on the front of the chest, in cases of empyema; but the formation of such an extensive abscess on the back, and over and beneath the scapula, must be a rare occurrence, if, indeed, it has ever occurred at all.

In this case, the tumour was of large dimensions, and could not be emptied by pressure; and I believe the true explanation of the case to be that given at the heading of this article, viz.—that this

was a case of empyema occurring as the result of a subscapular abscess having burst into the cavity of the chest.

6.—*An Account of Four Cases of Strangulated Hernia, each of which presented some point of interest.*

The first case of strangulated hernia to which I shall refer occurred in August, 1858. A gentleman, named H. H., had performed a long journey, rapidly, on horseback; and on some portion of the road, on making a sudden turning into a negro village to avoid a falling shower, he felt something "give way" in his right groin. On his return home he felt sick at the stomach, and complained of great pain in the region before referred to. I saw him next morning, and immediately discovered a strangulated femoral hernia on the right side.

I tried the taxis, and manipulated the tumour for some time, but it was immovable, tense and painful as it was. These symptoms being increased by constant vomiting, he could hardly bear it to be touched. I then tried a tobacco enema, giving him at the same time small doses of tartar emetic and opium, often repeated. A warm bath, in addition to the foregoing, and in which he was kept for a considerable time, helped to lower him, and to relax the spasm, and he lay weak and helpless on the bed.

Again I tried the taxis. I now distinctly felt that some portion of the tumour passed up into the abdomen, but the remaining part, and that the greater, remained fixed in the femoral ring, and could not be moved therefrom by any means other than operative.

He still suffered from excessive pain, vomiting, and all the symptoms of strangulation, and his bowels continued to be constipated. Accordingly I determined to cease any further efforts at reduction by the taxis, and I proposed, the aforesaid symptoms continuing, to operate. But this he would not hear of until a consultation could be held, and we had to send thirty miles in one direction, and twenty in another, to obtain assistance. In the meantime I had to content myself with giving this gentleman opiates to quiet him, and to enable him to obtain some rest. My two medical friends arrived early the next morning, one being Dr. Robert Oates, who practises in the lowlands of this parish (Clarendon), and the other a countryman of mine, whose name I mention with respect, viz., Dr. Frazer, an army surgeon, then connected with the Second West India Regiment, stationed in Spanishtown.

I should mention here that the exhibition of opiates gave my

patient a fair night's rest, and that by morning, and before my medical friends came, he was in a very different condition to what he had been on the day before. He was stronger and in much less pain, and vomiting had ceased. There only remained the tumour as before, tense and painful to the touch, and the bowels had not been evacuated. I thought, however, that the good symptoms counterbalanced the bad, and I determined to put in my veto to immediate recourse to operative measures being attempted, until a bold effort had been made to find out whether or not I had returned the intestine the day before. I became now very strongly of opinion, from the fact that vomiting had ceased, and the remembrance that I had returned some portion of the contents of the tumour by the taxis, that this had been originally a case of entero-epiplocele, and that I had returned, probably, as much of it as consisted of intestine, leaving omentum simply behind to be dealt with.

As I said before, however, there remained a tense, painful, and immovable tumour, an abdomen swollen from tympanites, and tender on pressure, and obstinate constipation. I mentioned my views in regard to the return of the intestine, and stated that from the cessation of the more urgent symptoms, and especially of vomiting, and the increased strength of the pulse, I was fully of opinion that the strangulation of the gut, at any rate, had been removed. I proposed then to my friends, on their arrival, that we should give Mr. H. a heroic dose of castor oil, in half a tumbler of champagne, as this would prove in a very short time, whether the bowels were patulous or not. This was agreed to, and within an hour my prognosis was fully verified. The oil acted enormously, and removed quantities of pent up wind and feces from the intestinal canal.

Here was, without doubt, a case of entero-epiplocele, converted by the taxis into a case of epiplocele, which, although painful and troublesome, is not, by any means, so dangerous or alarming as where the intestine itself is strangulated. The tumour became hot, swollen, inflamed, and very painful, causing fever, and keeping this gentleman in his bed for many days after. Leeches, locally applied, however, and a full course of mercury with opium warded off peritoneal inflammation, while change to the sea-side restored him to ultimate health.

I should add that, after some months, the tumour became entirely absorbed. The femoral ring, however, remains to this day entirely blocked up, and closely sealed against the possibility of any future

descent of a hernia, and I need not say that the irksomeness of a truss is dispensed with.

The second case was one of strangulated inguinal hernia, in which symptoms of strangulation of the intestine continued after the hernia had been reduced, and the intestine returned to its place in the abdominal cavity; operative measures, for the relief of the patient being ultimately required, and the case itself terminating in complete recovery.

A young negro stonemason, aged about twenty-three, had been lifting a heavy bar of iron, when he felt something "give way" in his side. A tumour appeared in the inguinal region, and soon after all the symptoms of strangulation set in. He was removed to Chapelton, and I saw him some hours after.

He had direct inguinal hernia. I tried the taxis, and succeeded, after a time, in reducing the tumour, and in freeing the man from the very great pain from which he had been suffering. Having applied a pad and a bandage, and given him an opiate, I took my leave.

Two days after I was again sent for, when I found this man, whom I had fully relieved two days before, suffering again from all the urgent symptoms of strangulated hernia. He was uneasy and restless, while his abdomen was hot and painful to the touch. He had constant vomiting, and he complained of an acute dragging abdominal pain. His pulse was small, sharp, and quick, and his face wore a very anxious expression.

With regard to the tumour itself, it appeared to have returned to a small extent, for I could feel a firm, inelastic protrusion at the abdominal ring. This was not, therefore, a case of reduction *en masse*, for the hernial tumour was still there, and now immovable. The finger could not be passed into the abdominal ring, as this was entirely blocked up by the protruding tumour.

There was no use now in attempting the taxis a second time, and, accordingly, I had no other course left to me than immediately to have recourse to the knife. Here, I would mention, the extreme delicacy of the position in which I was placed; and, unfortunately for myself, it is by no means an uncommon one in the mountains of Jamaica.

I could not call in anyone to advise with, or one with whom I could share the responsibility of the case. Even the village dispenser, Mr. Henry Lyon, who is frequently a great help to me in keeping up the administration of chloroform in operations,

was sick, and I had to cut this man for strangulated hernia in a dark room, and positively without any other help than that of brute force to hold him down in case of struggling.

Having given chloroform, I laid open the parts in the usual way until the sac became exposed, as carefully, and at the same time, as rapidly as possible. No intestine appeared to be included in the sac, and I freely laid it open, giving vent to about an ounce of bloody serum which it contained. The finger passed up easily through the abdominal ring, into the abdominal cavity, and I was positively at a loss to know, at this stage of the operation, why I had operated.

I only found, what has often been found before in such cases, viz., a quantity of fluid in the hernial sac, which had been secreted in the latter after the reduction of its more important contents two days previously.

On enlarging the opening, however, and passing my finger freely into the abdominal cavity, I hooked down something which came within reach; and this proved to be very healthy-looking intestine. I drew this down freely, and examined it carefully; when, some inches up, I found, to my great relief (for I began to fear that I had performed a needless operation), a deep sulcus separating one part of the gut entirely from the other. The intestine was much congested and looked, as if it had been tied, at this part, with a tense cord. It bulged out on either side of the constriction.

This band being carefully removed by a blunt pointed bistoury passed beneath it, the intestine resumed its usual shape, and was easily returned into the abdomen. At this part of the operation I found the poor fellow looking me full in the face, fully recovered from the effects of chloroform. He complained of no pain; but made a remark, which, while it showed that he cared for my anxiety about him more than for his own condition, caused the negroes present, and even myself, to laugh. Without preface, he called to some one standing by, "Wipe the doctor's face." I presume I was pouring with perspiration in that hot room. This is an instance of how a negro can bear pain; but, again, I have seen them scream and yell for little reason.

The wound in this man's groin healed up entirely by the first intention; and, within a week, he was up and about.

He recovered perfectly; and he has never since had occasion to wear a truss.

The other two cases I shall only refer to casually. One was

that of a gentleman, Mr. MacC., aged thirty-two, a professional photographer, who happened to be visiting Chapelton in October, 1865, at the time when I and the other gentlemen of Upper Clarendon were keeping watch, night and day, at the Court-house, as volunteers, for the protection of our homes, and of the honour of our wives and children, during that terrible ordeal through which Jamaica passed. Mr. MacC. had ridden down to the river to bathe, without having taken the usual precaution of putting on his truss—for he had an inguinal hernia previously—when, on making use of exertion, a large mass of intestine came down into the scrotum, and soon became strangulated.

I was quickly with him, for he lodged very near the Court-house, on the floor of which I had to sleep for many a night during the rebellion; and I tried the taxis, but in vain. He vomited incessantly; and the scrotum swelled out to such an extent, the wind gurgling through its contents, at each sickly gulp of the weak man, to such an extent that I thought the inflated intestine would have burst.

Again and again I tried the taxis, and failed. A stout drill-sergeant placed him with his heels over his (the sergeant's) shoulders, at my suggestion, in the hope that the intestine might, in this way, be drawn back into the abdominal cavity—but in vain.

I now made Mr. MacC. lie with his hips against one of the bottom posts of the bed, both his legs being supported against this post, between it and the wall. His legs were thus at right angles with his body; and he was placed in the most favourable position for the taxis, or for the gradual return of the contents of the hernial tumour. I left him for a little, and, having given him an opiate, he had some rest; but, in the meantime, the stricture had become relaxed; and a portion of the protruded intestine had returned, by means of its own weight, into the abdomen. The remainder was soon easily returned by the taxis.

The fourth case bears no point of interest, except that the plan of placing the patient in the position described in the preceding remarks, in this instance with one leg only, on the affected side, placed against the bedpost, enabled me to return an inguinal hernia as large and as hard as a cricket-ball, into the abdomen.

I never saw a hernial tumour so hard and unyielding as in this case; and yet the plan adopted, without any help from chloroform, tartar emetic, tobacco, the warm bath, or any other auxiliaries,

enabled me to overcome all difficulties, and to reduce this hard and exquisitely painful tumour, in a few minutes, by the taxis alone. Thus, a most unpromising case became, by this simple expedient, one very easily managed indeed.

ART. XI.—*On the Diagnosis of Renal from Ovarian Cysts and Tumours.* By T. SPENCER WELLS, F.R.C.S.; Surgeon in Ordinary to Her Majesty's Household; Surgeon to the Samaritan Hospital; &c.

DURING the few years which have passed since the revival of Ovariectomy in England, great attention has been paid to the diagnosis of abdominal tumours; and a knowledge of their distinctive characters is becoming so general, that mistakes which were formerly not uncommon are now comparatively rare. The diagnosis of ovarian tumours from pregnancy—from fibroid or fibro-cystic tumours of the uterus—ascites—tubercular or cancerous affections of the peritoneum—other forms of intra-abdominal cancer—hydatid cysts in the liver—iliac or pelvic abscess—is made repeatedly, in hospital and private practice, with a readiness and certainty which render a mistake quite an exception in a long series of accurate opinions. But exceptions still occur; and men of great experience must occasionally admit that an exact diagnosis is impossible. In other cases, it is only after an explanatory or incomplete operation, or after the death of the patient, that a mistake is discovered, and the means of avoiding it for the future are learned. Such a case has recently occurred in my own practice—a cyst, believed to be ovarian, proving to be a cystic degeneration of the kidney; and I avail myself of the earliest opportunity of bringing this case before the profession, with a few remarks on other cases in which I have known renal to be taken for ovarian disease.

The first case of the kind which came under my care was one of

SOFT CANCER OF THE RIGHT KIDNEY

in a girl only four years old. She was sent up from the country to me, in 1862, supposed to be suffering from ovarian disease. Her appearance is very well shown in the annexed woodcut, copied from a photograph, taken by Dr. Wright, while she was in the Samaritan Hospital.



The diagnosis in this case was made without much difficulty, although the urine was quite normal. The growth was extremely rapid; hardly six months from its commencement to its fatal termination—when the diseased mass weighed between sixteen and seventeen pounds. The tumour occupied the whole of the right side of the abdomen, bulging backwards in the right loin. It was uniformly elastic, but not fluctuant. The intestines were pushed downwards, and to the left side. The rapid growth, and the absence of fluctuation, were, of course, strongly against the opinion that the tumour was ovarian; while the rarity of ovarian disease in young children, and the comparative frequency of renal encephaloid, led to a diagnosis which was confirmed by a puncture with a fine exploring needle. A few drops of reddish serum were obtained, containing nucleated cells of most varied size and shape. I sent the child home, with a note, to Dr. Williamson of Nantwich, expressing my opinion that the tumour was a mass of soft cancer, and that the right kidney was the most probable seat of the disease. This proved to be correct. Dr. Williamson sent me the specimen, and I exhibited it at the Pathological Society, in December, 1862. The whole kidney was infiltrated with encephaloid. Although so enormously enlarged, the shape of a normal kidney was distinctly

preserved. Its surface was soft and elastic, in some spots giving a sense of deep-seated fluctuation; but no cyst was found, nor were there any marks of suppuration or hemorrhage. Coils of small intestine adhered to its inner and under surface. The ureter was completely occluded by the pressure of the tumour. The left kidney was quite healthy. Thus the normal condition of the urine was explained. The diseased kidney added nothing to the contents of the bladder, and the healthy kidney supplied only normal urine.

The following remarks on this point by Dr. Roberts, of Manchester (*Urinary and Renal Diseases*, p. 444), are well worthy of serious consideration. He says:—"The presence of cancer cells in the urine is a sign which usually figures prominently in the catalogue of symptoms of renal cancer, but its value is very doubtful. In all the later cases, especially when there was hematuria, the urine was carefully examined for cancer cells, but without success. Rosenstein mentions a case in which a cancerous villus was actually found projecting into the ureter, yet no cancer cells could be detected in the urine during life. It is by no means an easy matter to identify cancer cells in the urine, in consequence of their similarity to the transitional epithelium of the pelvis and ureter. . . . In two examples of renal cancer, with hematuria, which I have had an opportunity of observing, repeated and careful examination of the urine failed to discover the presence of cancer cells. Mr. Moore (*Med. Chir. Trans.*, xxxv., 466) believes that he succeeded in identifying cancer cells in the urine drawn after death from the bladder of a man in whose kidneys cancerous nodules were found; but his description rather accords with the appearance of the epithelial cells which are always freely detached from the vesical mucous membrane after death."

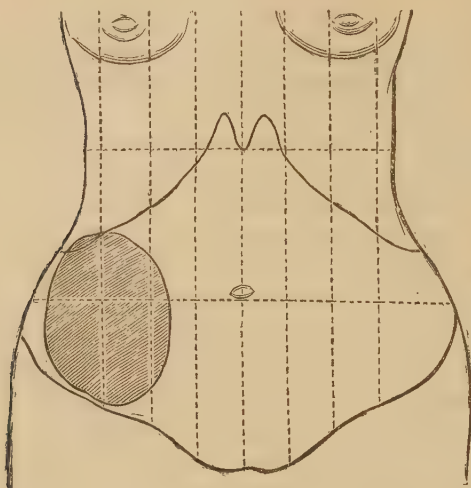
Whether renal cancer be observed in children or in adults—whether it be or be not accompanied by hematuria, or by the presence in the urine of albumen, or of epithelial cells from the ureter and pelvis of the kidney—whether the progress of the disease be slow or rapid—whether there may be much, little, or no pain, or emaciation, or gastric symptoms—or great or little effect upon the general health—the abdominal tumour is the most prominent characteristic of the disease. As Bright observed (*Abdominal Tumours*—Sydenham Society's Edit., p. 199):—"The enlargement shows itself much more towards the anterior part of the abdomen than towards the loins." It is, however, more or

less confined to one side of the abdomen and to the corresponding lumbar region, whence, as a rule it is immovable—and equally, as a rule, some portion of the intestines are fixed in front of it. But in one extraordinary case, an exception was found to both these rules. In the *Lancet* of March 18, 1865, a case is recorded in which an operation was commenced for the removal of a supposed tumour of the left ovary. The patient was in one of our general hospitals, and it was believed by the eminent physician-accoucheur who carefully examined her, and by the skilful surgeon who performed the operation, that “the tumour was ovarian, and that from its great mobility, and the absence of adhesions, its removal would be easy.” Yet the uterus and ovaries were found to be healthy, and the tumour to be the enlarged left kidney, which, instead of being fixed, was movable—its peritoneal covering being elongated into a sort of mesentery, admitting of free movements—and instead of pushing the intestines before it, the descending colon and sigmoid flexure were behind it.

The *absence of fluctuation* is the leading sign by which cancerous or other *solid* tumours of the kidneys are distinguished from ovarian tumours; for it is extremely rare to find a large ovarian tumour in some part of which fluctuation cannot be detected. But in some forms of kidney disease fluctuation is as evident as in ovarian cysts. It was perceptible in the second of the three cases on which this paper is founded—one of

HYDRONEPHROSIS OF THE RIGHT KIDNEY, WITH IMPACTION OF TWO CALCULI IN THE URETER.

On the 16th of May, 1865, I was hurriedly called to see the mother of a patient upon whom I had performed ovariectomy successfully, the daughter telling me that her mother had a tumour like that which I had removed from herself. I found the patient, about fifty years of age, in excessive pain all over the abdomen, but greater on the right side and in the right loin; and I felt a hard tumour between the right false ribs and the right ilium, reaching forward to within an inch of the umbilicus, as shown in the annexed diagram. The patient was so ill that I could not get any sort of history from her. I prescribed a full opiate, and directed it to be repeated in smaller doses at intervals of an hour until the pain abated—hot poultices being also applied. On the next day she was much easier, and I gathered the following history:—



She was fifty years of age; had married when twenty-two; had borne five children. Her last child was seventeen years old. Before this last confinement her health had been very good. This labour was very protracted, the presentation having been transverse. Ever since, she had been subject at times to pain in the back and right loin. It used to come on suddenly, increase in violence, and produce shivering and nausea. After six or eight hours it would cease. Her urine at the time of the attacks was usually thick, with yellowish sediment; at other times it was clear. For five years such attacks recurred pretty regularly every six weeks. Then, after a more active life, they recurred more frequently, scarcely a week intervening from one to another. In 1860 the catamenia ceased, and the attacks became milder and less frequent, and she was entirely free for a year or more. In 1862 the pains suddenly recurred with more violence than ever. After great suffering for several hours, "a dozen or two of little stones, as large as a pin's head," were passed with the urine. From this time till the present attack she had been quite well. On the 8th of May, 1865, while out walking, she stumbled and fell upon her abdomen. She was lifted up, complaining of great abdominal pain. She got home, went to bed, and next day the pain was so great that she was unable to get up. During the next six days she passed a good deal of blood in the urine, and she perceived, for the first time, a tumour as large as a cricket ball in the right side of the abdomen. On the 15th, the pain, which had almost ceased, returned suddenly with

great violence, and I was sent for. She was much relieved by the opiate prescribed; and I made a more careful examination of the tumour. It could be felt as shown in the diagram, but its margins could not be made out very distinctly. They appeared to be overlapped, on the right by the cecum, and the left by small intestine. Wherever the tumour could be distinctly felt, it gave a dull note on moderately strong percussion, but a clear one on deeper pressure and sharper percussion. By pressure forwards with one hand on the right loin, and the other on the front of the tumour, a trace of fluctuation was detected. Pain was kept in check by opiates, and on the 19th of May there was a prominent point near the middle of the tumour; fluctuation being distinct, I inserted a very fine trochar in this point (which was midway between the umbilicus and right anterior superior spine of the ilium), and drew off between two and three pints of thin pus, by a syringe attached to the canula by an air-tight joint. The urine, before the tapping, had been clear; but the day after, it was found by Dr. De Mussy to be loaded with pus. On the 21st the late Dr. Ritchie reported that it contained a large quantity of pus altered by the action of the urine. On the 27th, notwithstanding this escape of pus through the bladder, the tumour was as large as before the tapping. I therefore tapped again, and after removing two pints of pus, left the wound unclosed. There being no discharge after two days, I inserted a laminaria tent, having re-opened the wound with the lancet.

A very free discharge went on for the next fortnight. At first it was purulent, but afterwards it consisted of clear fluid, which was found to contain urea by Dr. Leared. The pain ceased, and the general health rapidly improved. The urine became clear and free from pus. On the night of the 17th of June some abdominal pain came on, but soon subsided, and the discharge from the opening suddenly ceased. Urine was passed with smarting, and was again found to contain pus, mingled with a little blood. Early in the morning of the 20th of June great desire was felt to pass water. After much difficulty and pain, a calculus of uric acid and urate of ammonia, as large as a broad bean, and much of the same shape, was passed, and was soon followed by a second, of similar dimensions. Relief was immediate. On the 25th a boil was felt just at the seat of the former punctures. On the 27th it burst, discharging about two ounces of grumous matter. The patient now felt so well that she was able to walk about and enjoy herself in the

country. On the 1st of July there was still a little discharge, perhaps one ounce in twenty-four hours. The abdomen was everywhere clear on percussion; but on deep pressure a hard painless tumour, as large as an orange, was to be felt in the right loin. After a few weeks this could no longer be felt, and the patient has since remained perfectly well. I heard of her in December, 1866, as being in better health than she had been for years.

This case is in many respects very instructive. The patient probably had a tendency to deposit uric acid before her last labour. The effects of that protracted labour perhaps led to the train of symptoms which ended, for a time, in the passage of numerous small calculi. Then, in 1863 or 1864, two renal calculi began to form, and set up chronic pyelitis. The fall in 1865 dislodged the calculi, and they blocked up the ureter. The pus and urine accumulated behind the calculi, and distended the pelvis of the kidney into the cavity from which I removed the large quantity of pus at the first tapping; and it was not till the calculi passed on into the bladder and left the ureter free that the formation of pus ceased and the artificial opening closed.

I have twice opened peri-renal abscesses in the loin, and in one case removed a small renal calculus through the opening. But the case just related is the only one in which I have punctured the kidney through the abdominal wall. It was clearly a hazardous proceeding, but the danger of rupture of the rapidly increasing sac into the peritoneal cavity appeared to be so great, and the suffering was so excessive, that tapping appeared to be less dangerous than expectation.

The following case of

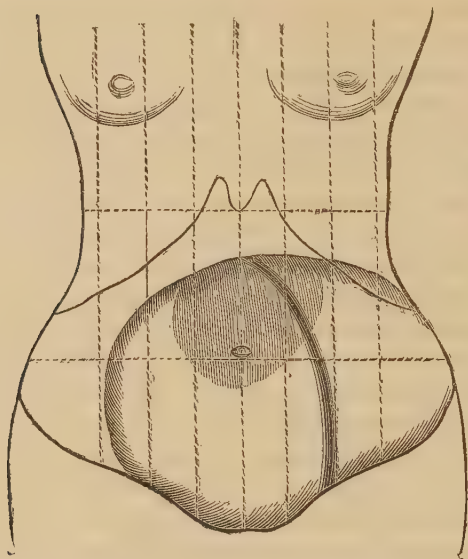
CYSTIC DEGENERATION OF THE LEFT KIDNEY

which was mistaken for a cyst of the left ovary, is not less instructive:—

On the 10th of October, 1866, a married woman, forty-three years of age, called upon me with a letter from Dr. M'Donnell, of Stoke Newington, containing a very full and accurate history of her case. She had been married 25 years, and had 9 children, the eldest being 23 and the youngest 4 years old. She had also had 1 premature birth, and 2 abortions; the last in 1861. Dr. M'Donnell wrote as follows:—"In April, 1862, she sought my advice for a hard swelling situated in the hypogastric and left iliac region, the size of an infant's head. Examination externally, and

per vaginam, convinced me it was an ovarian tumour. Mr. Solly confirmed this opinion on the 8th of May, 1863. In 1854 and 1855 a swelling was complained of, and had been the subject of conversation between husband and wife, but no advice was asked for at the time. Its situation was much as in 1863. Aching pain was felt, from time to time, in the tumour, without causing any alarm, from the time when it was first noticed by the patient herself. It had increased so much, in the early part of 1863, as to suggest the question of pregnancy. Some pain has, at times, been complained of in the lumbar region, and lower part of the abdomen, relieved by leeches, fomentations, &c. Leeches have been applied several times; the first time in November, 1863. In the Summer of 1863, the patient began to attend the Hospital for Women, in Soho-square, and became an in-patient in January, 1866, with a view to operation; but no operation was performed. She remained in hospital twelve weeks, her general health being then very bad, and she was much reduced in flesh and strength. After she left the hospital, the tumour increased in size, and extended to the epigastrium, and encroached so much on the chest as greatly to impede the breathing, and even prevent her moving about in bed. Assisted by Mr. Forman, of Stoke Newington, on the 4th of August, 1866, I withdrew, by tapping in the linea alba, two gallons of dark discoloured fluid, of the consistence of pea soup. The opening was made midway between umbilicus and pubes. The operation was well borne; the abdomen was entirely freed from fluid, the resonance being tympanitic everywhere, and no solid tumour to be felt in the pelvis. She recovered very favourably, and has been frequently out of doors since that time. The appetite, which had been entirely wanting for months previously, became, for a short time, very good. Her strength and spirits have much improved, though the cyst has re-filled."

It was rather more than two months after this tapping when I first saw the patient, and I then advised her to come into hospital before she became as much distressed as she had been before the tapping. She was admitted on the 17th of December, 1866. The tumour then occupied the position shown in the annexed diagram. At the upper and central part there was a patch of crepitus, giving the feeling of adhering omentum; and all down the front of the tumour, about an inch to the left of the umbilicus, was a cord-like ridge, which was taken by some who examined it for intestine, though it felt very like a large, long, and thick Fallopian tube. The



measurements were:—Girth at the umbilical level, 36 inches; from umbilicus to ensiform cartilage, 9 inches; to symphysis pubis $7\frac{1}{2}$ inches; to right ilium, 9 inches; and to left ilium, $9\frac{1}{2}$ inches. There was some mobility in the tumour, both vertically and laterally. Fluctuation was distinct across the whole tumour, in all directions. The left loin was dull on percussion, the right tympanitic. The uterus was high; the os hard and fissured, admitting the tip of the finger; the cervix short. No part of the tumour was below the brim of the pelvis. The catamenia was expected in a few days. They occurred regularly every three weeks—lasting five days. Dr. Junker examined the urine, and reported—“No albumen; deposits—urates, mucus, and epithelium.” She was subject to occasional nervous attacks, during which she was partially unconscious. She said they began by palpitation. She had four while in hospital, but they were regarded as hysterical, and attracted but little attention. The heart and lungs appeared to be healthy. The catamenia came on, and lasted a week, ceasing on December 29th; and on the 3rd of January, 1867, chloroform having been administered by Dr. Junker, I made an incision five inches long, extending downwards along the linea alba, from one inch below the umbilicus. On opening the peritoneum, I at once found that the hard roll, or ridge, observed running down the front of the tumour, was part of the transverse and descending colon, adhering

closely by means of the meso-colon and omentum, both to the cyst and to the abdominal wall. I separated some of these attachments, in order to tap the cyst safely. On introducing the trochar, about fifteen pints of fluid escaped. It had the appearance of pea soup. When the cyst was empty, I made some further separation of omentum and intestine; and when passing my hand round the right side of the cyst, what appeared to be another cyst gave way, and between one and two pints of clear fluid escaped. I then found that the deep attachments of the cyst were too close to admit of separation; and after tying three vessels which were bleeding in the separated omentum, and cutting off the ligatures short, I closed the wound.

The patient rallied slowly from the chloroform, and complained of pain, which was relieved by an opiate. Two other opiates were given at night—the total quantity given amounting to 50 minims of laudanum. Three hours after operation a small quantity of clear urine was drawn off by the catheter. After this not a drop of urine entered the bladder. At 10 p.m., the temperature was 98·4; pulse 116; respiration 28. The next morning the pulse was 120, and very feeble; skin dry; temperature 98; respiration 30. She was comatose, but easily roused, and answered questions sensibly. The coma gradually became more profound, and she died thirty hours after operation.

On examining the body seventeen hours after death there was no *rigor mortis*. The wound had united well. There were about four pints of blood-red serum, and a small tea-cupful of blood-clot in the peritoneal cavity. The right kidney was enlarged, and very soft; the cortical substance very friable, pale yellow in colour. The calices and pelvis were much dilated, and the thin sac formed by this dilatation had given way longitudinally. A calculus, weighing forty grains, was in one of the calices, forming a perfect cast of the calix. The bladder was contracted, and empty. The uterus and ovaries were healthy. The left kidney formed the cystic tumour which is described as follows by Dr. Junker:—

“The left kidney formed a cyst larger than an adult head. It presented one large cavity, composed of several wide pouches, arranged vertically at one side of the principal cavity. The stroma which formed the external wall was of varying thickness; thicker and stronger at the base of the pouches; thinner and less dense around the main cyst. It had a serous external coat; at some places

hypertrophied, at others atrophied. Next a fibrous structure (fibrous capsule of the kidney). This was followed by what appears to have been the cortical substance of the kidney, and from which portions could be traced into the septa (the former columnæ Bertini) which separated the pouches (the expanded calices). The main cyst (the original pelvis) was formed by the peritoneal and fibrous capsules. The medullary portion could not be well distinguished by the naked eye from the thickened lining membrane. Thus the tumour appears to be a good specimen of genuine hydronephrosis, in which pelvis and calices expand into a large cavity, and produce, by pressure, atrophy of the original structures of the organ.

“The peritoneal coat was rough with shreds of the broken down, extensive, and intimate adhesions. Some of the neighbouring organs, or portions of them, were so intimately connected with the tumour that their separation was impossible, and portions had to be cut off in order to remove the cyst. Such connexions existed between the spleen, the head of pancreas, the great curvature of stomach, principally at the pyloric end, the duodenum, a part of the left lobe of liver, coils of small intestine, omentum and mesentery, and along the entire extent of the vertebral column, as low as the second lumbar vertebra, uninterrupted, to these bodies, and their left transverse processes, and to the right transverse processes of most of the dorsal vertebræ. No adhesion, however, existed between the tumour and the bladder, uterus and its appendages, or the rectum.”

After the information obtained by the *post mortem* examination, I made further enquiry into the history of the case, especially as to the state of the urine, and I learned from Mr. Scott that while the patient was under his care in the Hospital for Women, in January, 1866, the urine contained pus and albumen, was alkaline, and of low specific gravity, about 1.005. He had “no doubt of the tumour being ovarian, but considered the case an unfavourable one for operation, believing the front of the tumour was crossed by a loop of intestine which would, in all probability, be firmly adherent throughout its course; from the certainty of considerable adhesion, in consequence of the repeated attacks of inflammation; and from the presence of pus and albumen in the urine, with a feeble circulation. The quantity of pus varied considerably during her stay in hospital; albumen was pretty constantly present.” Dr. M'Donnell has ascertained that when twelve or fourteen years old she was struck by an iron shovel with great

violence on the abdomen, near the left ilium. "She was felled on the spot, and remained insensible for some (indefinite) time. She was ill afterwards, and attended at St. Bartholomew's and other hospitals for eighteen months as out patient. She told her husband that during all this time she 'suffered much from the urine,' but did not explain more precisely the nature of the suffering; for four or five years subsequent to the first period of eighteen months, and for a like period during the first years of married life, she suffered pain and distress, referred to this injury. Her pregnancies were always attended with distress—indeed during her whole married life, 26 years, she repeatedly suffered from deep-seated pain in the abdomen where the injury was inflicted."

It is evident from the cases just narrated that both solid and cystic tumours of the kidney may be mistaken for ovarian tumours. Solid renal tumours, whether cancerous or innocent, may resemble the malignant, pseudo-colloid, or cysto-sarcomatous tumours of the ovaries; while different varieties of ovarian cysts may be closely simulated by different forms of pyelitis and pyonephrosis, hydro-nephrosis, cystic degeneration, and the growth of hydatids in the kidney. Perhaps the diagnosis may be facilitated by attention to the following propositions:—

1. Although intestine is sometimes found in front of ovarian tumours, and sometimes behind movable renal tumours, these are very rare exceptions to the general rule that renal tumours press the intestines forward, and ovarian tumours press them backward. In other words, ovarian tumours are in front of the intestines, renal tumours are behind the intestines.

2. Large tumours of the right kidney usually have the ascending colon on the inner border of the tumour. Tumours of the left kidney are usually crossed from above downwards by the descending colon.

3. The discovery of intestine in front of a doubtful abdominal tumour, should lead to a careful examination of the urine. It is possible that one kidney may be diseased and the urine quite normal, because the healthy kidney alone secretes urine. But the rule is that either blood, pus, or albumen, or characteristic epithelium, are detected—or some history may be elicited of their being detected at some former period.

4. If any doubt be entertained whether a substance felt between an abdominal tumour and the integument be or be not intestine, percussion may not solve the doubt, because the intestine may be empty and compressed. But (a) an intestine when rolled under

the fingers contracts into a firm, cord-like, movable roll; (*b*), the patient may be conscious of the gurgling of flatus along it, or the gurgling may be heard on auscultation; (*c*), the intestine may be distended by insufflation, after passing a long elastic tube through the rectum.

5. Ovarian and renal cysts may both be subject to great alterations in size. When the kidney is the seat of disease the fluid usually escapes by the ureter and bladder. An ovarian cyst can only empty itself through the bladder after adhesion and a fistulous opening. It may discharge through the Fallopian tube and uterus, or into an intestine, or through the coats of the vagina. In either case the physical and chemical characters of the fluid discharged will be the chief guide in diagnosis.

6. If a correct history can be obtained, it may be expected that a renal tumour has first been detected between the false ribs and ilium, and that it has extended first towards the umbilicus, next into the hypochondrium, and lastly downwards towards the groin. An ovarian tumour has, in all probability, been first noticed in one inguinal or iliac region, and has extended upwards and inwards.

7. It is only a very small ovarian tumour, with a long pedicle, which could be mistaken for a floating or movable kidney. The latter may be recognized by its characteristic shape, though it is often so misplaced that the hilus is turned upwards. The kidney is usually felt between the umbilicus and the false ribs, and may be pushed upwards and downwards, or laterally, to a varying extent, or into the lumbar region to the normal position of the kidney. When the kidney is pushed away from this position the sound, on percussion, becomes tympanitic.

8. Just as renal tumours are usually associated with some evidence or history of hematuria, calculus, albuminuria, nephritic colic, or some notable change in the quantity or state of the urine, so ovarian tumours are usually associated with some change in the quantity and regularity of the discharge, or with suffering at the catamenial periods, and with some alteration in the mobility or situation of the uterus. But as in some rare cases of renal disease the urine may be normal, so in some rare cases of ovarian disease there may be nothing abnormal to be discovered in any of the pelvic viscera, or in their functions.

By bearing these facts in mind an accurate diagnosis may be made in a very large proportion of cases. Some rare cases of exceptional difficulty may, however, be occasionally expected.

Not as an excuse to the careless and ignorant, but as some solace to others who have erred unwittingly, and as an answer to some who, having little experience of the difficulties of actual practice, are apt to speak of all mistakes as inexcusable, I quote the following remark of one of the greatest clinical teachers of any age or country—Bright:—"I have known the enlarged kidney to be mistaken for disease of the spleen—of the ovary—of the uterus—and for a tumour developed in the concave part of the liver; *nor is it, perhaps, possible, by the greatest care and the most precise knowledge, altogether to avoid such errors.*"—*Op. cit.*, p. 198.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Rapport au Conseil de Santé des Armées. Sur les Resultats des Service Medico-Chirurgical aux Ambulances de Crimée et aux Hôpitaux Militaires Français en Turquie pendant la Campagne d'Orient, en 1854, 1855, 1856. Par J. C. CHENU, Docteur au Médecine; Médecin Principal. Folio, pp. 732. Paris: Victor Masson et Fils. 1865.

FULLY ten years have elapsed since the fall of Sebastopol, the crowning act of one of the most memorable campaigns of modern times—more splendid for the vanquished than the victors; and the history of the great events of the years '54, '55, '56 is yet to be written.

As a valuable contribution to the medical and surgical history of the war in the East the Report of M. Chenu will prove acceptable to all, more especially to those who took part, in person, in the events which he describes in such fulness of detail.

M. Chenu insists, at the outset, on the great importance of duly estimating the effects of disease on armies in the field; an important truth, which cannot be too often impressed on statesmen and commanders, who, willing enough to bring all a nation's resources to bear on the material of destruction requisite for campaigns of any magnitude, too often exhibit a spirit of parsimony in providing for the hygienic and medical wants of the troops engaged. Medicine has at all times striven to do her part; but until a post of higher authority is assigned at the council of war to the surgeon or physician-general of the army, we must still expect to hear of unnecessary sacrifice of life by fever or plague, and be content to believe that "the exigencies of military service" demand in each campaign a holocaust of perhaps the best lives which the nation prodigally furnishes from her most promising youth.

"War," says M. Chenu, "as military medical officers have not ceased to proclaim, and has been again recently well said by Dr.

Marrion, Physician-in-Chief to the Navy—war inspires generally in the world only the idea of combats, of strife more or less deadly. The soldier seems made only to be either killed or wounded. We forget too easily that the proportion of those who succumb to diseases, *due, in great part, to immaturity of constitution*, is infinitely superior to the number of those who are struck down by the fire of the enemy. We do not think sufficiently on those destructive scourges which fasten upon the flanks of an army perfectly provisioned with all the munitions of war, but always taken unprepared by those avalanche-like epidemics, which it would not be at all impossible to foresee, and the gravity of which might be moderated by measures which we shall indicate presently in speaking of recruiting and of the uniform *regimen* of an army in the field.”

Perhaps in no campaign on record do the statistics now available so entirely and finally prove the truth of the positions here laid down. In the pages of M. Chenu we find summed up, not alone the total forces engaged on all sides, with the losses in killed, wounded, and dead of disease, but a considerable statistical return of the absolute number of projectiles of all kinds employed in both the Russian and the allied camps.

Some idea may be formed of the magnitude of the medical requirements of the French army from, or on consideration of, the following statistical returns. The highest number the French army ever attained at any period of the war in the Crimea appears to have been 150,000 men. The return for January, 1856, shows a total of 144,000. To maintain the army in an efficient state a total of 309,268 men were, at various periods, sent to the field of action from France and Algiers.

The following table shows the total losses in the various armies engaged, as summed up by M. Chenu:—

	Killed	Died of wounds or disease	Total
French army, . 1854–1856,	10,240	85,375	95,615
English army, . 1854–1856,	2,755	19,427	22,182
Piedmontese army, 1855–1856,	12	2,182	2,194
Turkish army, . 1853–1856,	10,000?	25,000?	35,000?
Russian army, . 1853–1856,	30,000?	600,000?	630,000?
	53,007	731,984	784,991

The numbers of the Turkish and Russian armies are given only approximately. The details for the French, English, and Piedmontese armies are as follows:—

French Army, 1st April, 1854, to 6th July, 1856, and subsequent Deaths, to December, 31, 1857.

Total of troops sent out, 309,268.

	Entries into Hospitals or Ambulances	Killed, Died, or Disabled
Different diseases and cholera, from 1st April, 1854, to 20th September, 1854, ... }	18,073	8,084
Ambulances in Crimea and hospitals at a distance from Constantinople, ... }	221,225	29,095
Hospitals at Constantinople, ...	162,029	27,281
Killed by the enemy or disabled, ...	—	10,240
Died, not in hospital or ambulance, ...	—	4,342
Loss of the <i>Sémillante</i> { Troops, ...	—	394
{ Sailors, ...	—	308
Infirmaries and hospitals of the fleet, ...	34,817	846
Died in France after the evacuation of the hospitals of the East, up to Dec. 31, 1857, }	—	15,025
	436,144	95,615

Taking into account that in many instances the same sick and wounded must appear in the returns of consecutive hospitals, and the cases of the same individuals returning to the same hospitals once, twice, or oftener for wounds or diseases, M. Chenu estimates the actual total of wounded and sick at 225,000.

English Army, 1st April, 1854, to July, 1856.

Total of troops sent out, 97,864.

Wounded, ...	18,283,	—	of whom 597 officers.
Died in hospitals from wounds, ...	—	1,847,	of whom 86 officers.
Killed on the field of battle, ...	—	2,755,	of whom 157 officers.
Admitted to hospitals—fevers, cholera, dysentery, &c., ... }	144,410,	—	
Deaths in hospitals, ...	—	16,298.	
Evacuated from hospital, and }	—	1,282.	
Died at sea, ... }			
	162,693,	22,182.	

Amongst the deaths in hospital are to be noted 4,513 from cholera, and 463 from frost-bites. No account have yet been forwarded of those who must have died of the effects of wounds after the return of the army to England.

In the army of Piedmont, with a total of 21,000 men sent out, we find the return as follows:—

Killed by the enemy,	...	...	...	12
Died from wounds,	...	...	...	16
Died from disease,	...	...	...	2,166
				2,194

Let us now turn to the consideration of the means of destruction employed on all sides—and in no previous campaign have such ample materials been made available—to enlighten us as to the statistics of the agents of destruction, and the result thus produced as compared with those due to the invasion of disease.

The following table indicates approximately the number of projectiles employed in the Crimea by the various forces engaged, with the results in killed and wounded:—

	Projectiles employed	Killed or wounded
French army,	... 29,463,363	50,836
English army (?),	... 15,000,000	21,038
Piedmontese army,	... 50,000	183
Turkish army,	... 50,000	1,000
Allied fleets,	... 35,000	2,000
Russian army,	... 45,000,000	100,000
89,595,363		175,057

This gives a proportion of about one killed or wounded for every 500 projectiles (accurately, within a fraction of one to 512). We cannot very well see how to accommodate the figures with M. Chenu's statement that, deducting deaths by explosion of mines, powder magazines, and various other causes, the proportion is one killed or wounded to every 1,000 projectiles.

The excess of small-arm fire over that of guns of large calibre throwing round-shot, grape, &c., appears to be infinitely greater than what we presume would accord with popular estimate. It is estimated, in the returns of which M. Chenu has availed himself, thus during the campaign:—

The Russians fired 2,800,000 large projectiles, solid or hollow.
42,200,000 balls.

The French returns show that there were employed--

532,565 canon balls,	}	1,104,446
226,386 obuses, box shot, &c.,		
340,695 bomb-shells,		
4,800 grenades, or fusees,		
12,362,648 spherical bullets,	}	28,355,916
12,923,768 oblong bullets,		
2,379,116 Nessler bullets,		
690,384 balles evidées,		
		29,460,363

M. Chenu does not furnish any statistics to show what comparative amount of destruction was due to large and small projectiles respectively. Our own experience goes to show that, while shell and round shot produced, in numerous instances, very destructive effects, accompanied not alone with great loss of life, but much dismemberment and disfigurement of body, by far the largest proportion of deaths and wounds were due to the bullet.

Of the great skill of the Russian troops in the use of small-arms abundant evidence is on record. Not less can be doubted their energy and activity in immediately availing themselves of every improvement effected in the construction of instruments of offence and defence, and projectiles of all kinds. The following incident communicated to ourselves at the time, by a very distinguished medical officer at the head-quarters of the French army, may be cited as an apt illustration of what is here stated:—

The form of bullet known as the *balle Nessler*, and employed largely, as shown by the above tables, was the invention of an officer serving with the French troops in the Crimea. It was a bullet of short axis, consisting of a hemi-spherical head, very short cylindrical body, and deeply cupped base. The model of this projectile was, as we are informed, invented in the Crimea by M. Nessler, and sent to Paris to be manufactured in quantity for use in the field. Before the French supply arrived a ball of this identical construction was fired, and with effect, from the Russian side; and the writer has himself removed a *balle Nessler* (now in his possession) from a deep-seated wound of the neck. The conclusion is obvious, but the explanation not easy.

While the killed and wounded are seen to be as 1 to 500 for the projectiles employed, the killed and deaths from wounds show as 1 to 3·76 to the deaths from disease.

Inspection of the following table, compiled from M. Chenu's returns, show the total of troops, the total of wounded, of killed, &c.:—

Deaths from Wounds, with the Total of Deaths from Disease, for the Allied Armies.

—	Total Men	Killed	Wounded	Deaths from Wounds	Entries to Hospital	Deaths from Disease
French army, .	309,268	10,240	40,320	10,000	436,144	74,000
English „ .	97,864	2,755	18,283	1,847	162,693	17,580
Sardinian „ .	21,000	12	—	16	6,620	2,166
24,870 Deaths by Projectiles.					605,457	97,746

The results of these columns may be thus summed up:—

Proportion per Cent. of Deaths, to Total Forces Employed, by Projectiles and Disease.

	Per-centage of Deaths from Projectiles	Per-centage of Deaths from Disease
French army, -	6.54	23.93
English army, -	4.70	17.96
Sardinian army, -	0.13	10.31

We have seen that the proportion of deaths and wounds to projectiles was 1 to 500; but of the wounded many recovered. The exact proportion of deaths, including those dead of wounds, amongst the allied troops to the Russian projectiles employed is as 1 to 1,809—24,870 deaths by projectiles to 45,000,000 Russian projectiles.

Now, we find returned an aggregate of 93,746 deaths from disease, with a total of 605,457 admissions to hospital. Omitting 11,863 who died of wounds in hospital, and excluding then those who recovered of wounds in hospital (numbering), the admissions for disease may be taken at 593,594; the deaths from disease show a proportion to the number of admissions of 1 to 6.33.

To the siege of Sebastopol the allied hosts sent a total of 428,132 men; of these 118,616—more than one-fourth part—totally perished by disease and the engines of destruction of the enemy. Of those thus destroyed but 24,870 were slaughtered by the Russians, while 93,746 were annihilated by disease.

If we suppose the Destroying Angel hurling his death-shafts from the summit of the Malakoff Tower on the invading hosts, we might well conceive that even his immortal arm was wearied when he found that to bring death to one individual in the allied armies he had to hurl against him not less than 1,800 of the most deadly projectiles which the art and science of the most advanced and civilized nations of modern Europe placed at his command. Gladly could we fancy him turning to his older, surer, and more lethal weapons, fever, flux, scurvy, or aught else of his armoury of disease, which, for each six shafts sent forth, laid low his man; the sum of his work being that, for 45,000,000 projectiles expended, he produced a diminution of but one-sixteenth of the forces they were employed against, while, with somewhat over 600,000 attacks of disease, he cut off little short of one-fourth of the invading army, and disabled, for a period, every man in the field against him once and a-half times.

An interesting and not unimportant point of view in which M. Chenu discusses the bearing of the sickness of the army is in regard to the question of expense; and here, too, we have the elements for comparison as to cost incurred for the cure of the wounded and those from disease. Dead men tell no tales, it is said; and in the statistics of war they bear but little cost. The first loss is all that is felt when the bullet, the shell, or the bayonet has done its work effectively; the most serious item to every commander is "the butcher's bill" for the care, cure, or provision of the wounded until death or recovery, and often for life in the latter case.

Comparing the number of days required for the treatment of wounded and sick, we find, for the French army:—

	Wounded	Sick, not Wounded	Total
Days of treatment,	1,934,313	5,337,888	7,272,201

Estimating the cost of treatment at 2 francs 50 centimes per day, M. Chenu calculates the cost of treatment:—

For wounded, ...	4,835,772:50 =	£193,431	6	3
For sick, not wounded, .	13,344,720: 0 =	£533,788	16	8

A fearful loss is here involved; not altogether avoidable, it is true, yet far exceeding the limits to which, as we maintain, it should extend if all due precautions for the care and maintenance of the troops were exercised.

If we estimate the prime cost in money of each soldier sent on that campaign, when actually in the field equipped for service, at £200—a low calculation, be it observed, when made to include cost of transport, &c.—we shall find that the deaths on the field and from wounds represent a money value of about £4,974,000 sterling, while the deaths from disease represent a loss equivalent to about £18,749,200.

In accounting for the large mortality of the allied forces, M. Chenu dwells forcibly on the defects of the system of recruiting—the immature age, strength, and development of the young soldier—the imperfection of his organization, and his inability to withstand, for any lengthened period, the fatigues, hardship, and duties of campaigning. Of a force of 100,000 men, as recruited for the Crimean campaign, he believes that at least one-tenth will be found ineffective. The total French force present at the Alma was but 30,000 men, more than 18,000 having been admitted to hospital before seeing the enemy or having had to make a forced march. From the English statistics he cites the already published statement that of upwards of 94,000 sent to the seat of war, more than 15,000 are reported as unfit for military life when recruited, owing to various defects of constitution. In the report to the Minister-at-War, subsequently published by Parliament, Dr. Lyons, the Pathologist-in-Chief to the British Army in the East, points attention, in the strongest manner, to the incompleteness of physical development, and consequent inherent unfitness for service, of a very large portion of the recruits sent out during the war. The following passages may be cited here with advantage:—

“In treating of the various predisposing causes of Disease in operation amongst Her Majesty’s Forces in the East, there is one which may be classed amongst Physiological influences, and which demands most earnest consideration at your Lordship’s hands. I allude to the extreme youth, incomplete physical development, and general immaturity of a very considerable proportion of the strength of several regiments, and constituting, as I have reason to believe, in the total of the Army, a number sufficiently great to render the question one of vital moment and of even national interest, as largely affecting what may be called the Physiological Economy of the Army. Amongst even well-matured constitutions, the hardships and fatigues, trials, privations, and exposure of campaigns, such as those of the past years in the Crimea, must almost of necessity prove largely productive of disease, and induce much mortality. But on the undeveloped frames and the unripe strength of the ill-seasoned

recruit such causes operated with twofold energy, and with a more than doubly fatal effect.

“Not only does the power of resistance to morbid influences, and the consequent invasion of Disease in such youths seem inferior to that of the older soldier, but when attacked they succumb more readily under the effects of a Fever or a Flux. And even when such disease does not prove immediately fatal, the convalescence of the young is generally slow, constantly attended by a disposition to relapse, or to the development of those Secondary Enteric Lesions the almost unfailing result of such Fevers as those which prevailed amongst the Troops in the East. Again, should constitutional taints exist, as is not uncommonly the case, more especially in regard to the Strumous diathesis so general in some of the classes from which recruits are largely raised, the fatigues and privations of Active Warfare, the exposure and vicissitudes of the field, and the attacks of Acute Disease, supply precisely the stimulus most calculated to call into active operation various low and complex forms of Disease with a slow and lingering course. Such diseased processes either prove directly fatal of themselves, or so complicate other morbid states set up in the system, as to leave no expectation of aught but a fatal issue.

“Such doubtless is the rationale of the Pathology of many of the examples of Tubercular development in Typhoid and Dysenteric cases, which we have witnessed in the Secondary Hospitals of the Bosphorus, in some instances causing death by Lesions peculiar to itself; in others inducing most complex processes of disorganization in the vital parts.

“Even when the less formidable of such cases are not actually fatal, convalescence is usually so slow, lingering, and imperfect; that as soldiers, these youths cannot be, till the expiration of a long period, if ever, sound and effective men; while during such time of convalescence they but burden the Hospitals, required as they often are, for the more pressing wants of the field. Thus not only does their illness cause the loss of their own services, but it encumbers the Hospital Staff, while their transport is an additional pressure on departments already perhaps, fully worked.

“The results of Surgical Pathology corroborate to a large extent the observations just made, which are chiefly based on considerations arising out of the behaviour of the immature constitution under the effects of Disease. As a general rule, true at least in a very large part of its usual acceptance, youth forms a favourable element in the calculation of chances in surgical cases. Amongst the troops in the Crimea, however, no such favourable anticipations could be indulged on the score of youth. The constitution of the young, even independently of the presence of actual disease, seemed much impressed by the influence of the various causes already dwelt on; many succumbed almost immediately under the shock of injuries, and in the case of the graver Surgical Operations, no more advantageous results, but the contrary, appeared to be shown on the side

of the young: in fact, youth was not to be counted on as a favourable element in determining a prognosis in such cases.

“In the statements I venture to make respecting the operation of youth and immature physical development as a predisposing cause in the production of Disease, I do not rely solely on the results of my own observations. This subject has been the topic of repeated consideration with some of the most experienced and accomplished medical officers in the field, by whom my views on these physiological points have been fully concurred in. And, indeed, I do not believe that any persons sufficiently acquainted with the principles of Physiology could entertain opinions in the least differing from those here expressed.

“I am not in a position to prove, by actual statistical evidence, the precise proportion to strength in the entire Force in the Crimea, at any given date, of those who may be considered as of immature age. I did not possess the means of determining such questions on any large scale; and I do not pretend to any such results. However, amongst a portion of the wounded of the 18th June and 8th September, I found means to make a few statistical observations. The cases may, I think, be fairly taken as a general average, and so far admit of being used to support what is stated above. The joint numbers on the occasions of the assaults on the Grand Redan, received into one hospital in the field, amounted to 664, embracing all variety of gun-shot injuries, and some of the very worst kind. Of this number of men the average age was $24\frac{1}{2}$. But this statement would give only an inadequate view of the actual youth of a great proportion of them. Sixty-one were *under* the age of 20, of whom one was only 17, and the rest varied between 18 and 19; 75 were *of* the actual age of 20 years. Thus of the whole, considerably more than one-fourth were actually only 20 years of age and under; 76 were found to be between 30 and 40, inclusive; two were of the actual age of 40; while one was stated to be 48, which is at least doubtful. The remainder, constituting a little more than two-thirds, were of various ages between 20 and 30, the majority inclining to the former rather than the latter. These results show a very high proportion of immature youth, such as, I have no hesitation in saying, it is not consistent with any sound physiological principles to expose to the severe trials and hardships of actual War. Amongst them disease finds a rich and easy harvest.

“I would not wish to be understood as implying that any objections exist to the practice of early enlistment. On the contrary, such a measure is undoubtedly of great general utility. On home service, or on secondary foreign stations in favourable climates, the training and well-regulated exercise of the young soldier conduce to the development of a vigorous frame, and in no small degree add, I am persuaded, to the natural powers of the constitution. Such training should, however, be gradual and apportioned to the strength of the recruit, according to the earliness of

the period at which he is enlisted ; and under a properly regulated system I believe it would not be necessary to fix any precise limits in this direction. Trained at home, and gradually inured to the fatigues of the soldier's life, by a suitable rotation in the selection of foreign stations, Troops could, I doubt not, be slowly but steadily brought to a maximum of safe endurance of the hardships of Warfare, far above the average of what is now reached. A judicious system of 'cross drafting,' or some such expedient, would enable the younger soldiers and recruits of one regiment ordered to the seat of War to be replaced by the more hardy, seasoned, and serviceable men of another ; while in the several secondary depôts on the high road to the scene of action, the former could undergo a gradual process of seasoning and acclimatization, which would ultimately add immensely to their actual strategic value and efficiency when called to the field in their turn. In this way I believe no loss of strength or actual inconvenience to the Service could result ; while the increased efficiency of a large proportion, probably one-fourth of the Forces, in the field would be thus almost certainly secured. I am sufficiently well aware that the employment on active service of the young and immature prevails largely in other Forces besides the English ; but I have reason to know that it is with equally unfavourable results. Neither precedent nor practice, however, can be admitted as outweighing the dictates of Physiology, and the results of Pathological experience in a matter of such moment. As a conviction based on these considerations, I would beg respectfully to express it as my unqualified opinion that no soldier under 21 years of age should be sent on service to the Crimea, or any other seat of actual War : and not even then, without a previous training and gradual acclimatization in suitably chosen secondary stations. In the foregoing statements I believe I am justified in saying, that I express views shared in by a large number of the most able and intelligent Medical Officers.

Besides the class of men who, by reason of youth and immaturity of physical development, seemed to suffer peculiarly from the toils of Crimean life, it will be necessary to briefly notice another class equally unfitted to make effective Troops, but from a somewhat different cause. I allude to those recruited at a rather advanced age, 25–30, from the mechanical trades in densely populated towns. Such men have seldom enjoyed a vigorous youth ; and even at this age (30) the meridian of their physical development is passed. Ill fortune, or it may be a life of dissipation, has led them to take to arms as a last resource. How far any of such men are capable of becoming efficient soldiers I know not ; but it is certain that the testimony of Medical Officers bears me out in the observation that they are constant and early applicants for admission into hospital, their constitution, never robust, soon giving way under exposure and fatigue. Men of such a class may add to the numerical, but never

to the effective strength of an Army, and it is more than doubtful if they ever repay the labour and cost of their transport, and of their maintenance in the Field or Secondary Hospitals. One sound, seasoned, and effective soldier outweighs in value any number of immature and inefficient youths or effete adults. And in the Physiological Economics of an Army, the maintenance of the largest number of effective men is the great problem.

“The several agencies just considered, many of them continuously in operation, and others, though ceasing to be immediately felt, leaving well-defined impressions behind them, had induced sensible modifications, as I have already said, in the constitution of the soldier. Some of these effects were visible to the eye. A marked characteristic of the Crimean soldier was a premature appearance of age. The veteran of 40 would be readily taken, by the inexperienced, for the old man of 60 to 65; the youth of 20 to 22 wore the settled aspect of 35 to 40. With the haggard features of disease, especially that of a chronic kind, these appearances became still more exaggerated, often to a most marked degree.

“Even with those who possessed a still robust exterior, and all the apparent physical conditions of health, the manner in which their system was observed to behave under the influences of disease or the effects of injury or operations, showed that the force of the constitution was to a very considerable extent deteriorated. To express this in another way, it may be stated that the available *vis vitæ* for the resistance to, or recovery from Disease, or for the resistance to, or recovery from the effects of Injury or of great Surgical operations, was below the average physiological standard. The powers of the system, and consequently the *viability*, so to speak, of the soldier were below par. This was especially manifested in some of the prominent and distinctive characters of prevailing diseases, which presented a decidedly low adynamic, and often Typhous type, and such as urgently called for a generous and stimulant treatment.”

The valuable observations of Professor Aitken,^a Drs. Liharzik, Boyd, Dawson, &c., may be studied with advantage, as bearing on the question of mature growth and fitness for laborious occupation. The human adult, it will be found, does not come to his fully matured growth until he attains the age of twenty-five years. In the forcible language of Lord Raglan to the Duke of Newcastle, during the height of the Crimean campaign:—“Those last sent out were so *young* and *unformed* that they fell victims to disease, and were swept away like flies.” That too great a standard of height was aimed at in the English service there can be now little doubt,

^a See Professor Aitken's *Growth of the Recruit.* 1862.

that for growing lads of eighteen having been above that of the average human being, as shown by the researches of Quetelet, Liharzik, &c. We question if even yet too high a standard is not exacted in the British army. Well-knit *little* men will be found to stand the wear and tear of the soldier's life better than those more showy in appearance because of greater height.

An interesting portion of M. Chenu's report presents us with a journal of the daily states, numbers, positions, operations, casualties, and diseases of the French army throughout the entire campaign; appended to which we find, in reference to all the great operations jointly undertaken, a summary of the similar records furnished for the British army; and when the Sardinian army became engaged like data are cited for that force.

At a very early date aberrations of health began to be noticed in the French troops. Works connected with the projected defence of Galipoli commenced 10th April, 1854, and necessitating trenching and turning of earth, and employing about 3,000 men, appear to have been the principal cause, in conjunction with cold night air, in producing fevers and ophthalmia. An extra ration of brandy was now issued. Scurvy began to make its appearance in the fleet April 23, 1854; cholera made itself felt in the succeeding month; paludal influences likewise began to exhibit their effects, especially amongst the sailors of the fleet, in the induction of intermittent and remittent fevers. M. Chenu, we may here observe, with regret, supports the view that cholera was brought from France to Galipoli, and thence by certain convalescents to Varna. He goes so far as to name the individual ships by which the chain of contagious communication was, as he conceives, established between the ports of Bulgaria and those of the south of France. Cholera undoubtedly prevailed at Marseilles at the period in question.

Towards the close of July took place the expedition into the Dobrudscha, so unfortunately signalized by a destructive invasion of cholera which speedily necessitated the abandonment of an advance in that direction. The effective mean of the French troops for this month (July, 1854) represents 55,000 men—8,239 cases of cholera are reported, of which 5,030 died; the month of August furnished a less number of cholera cases, but proportionately a far higher mortality, viz., 3,043 cases, with 3,015 deaths. Thus, before the landing in the Crimea, a total of 18,073 French soldiers had passed into the various hospitals of Galipoli, Adrianople, Varna, and the ambulances, with a mortality of 8,045, caused by cholera alone.

History will take but little notice of this terrible scourge of the French army, although she will never weary to tell of the events which occurred twenty days later—the battle of the Alma, 20th September, 1854—when the killed in all armies engaged—141 on the French side, 348 on the English, and 1,807 on the Russian side, in all 2,296—bears but a small proportion to the total of deaths garnered up by disease amongst the French alone in two short months, viz., 8,045! and the total of killed and wounded in all the armies engaged on that memorable day, 8,036 (of the latter, be it observed, a large part recovered), does not quite reach the sum of the losses of the French army, before its landing at Oldfort, from one malady alone—cholera.

It will be at once convenient and not devoid of interest to insert here brief summary statements of the details of the casualties experienced by the armies engaged in the principal operations of the war. They will serve as points of comparison for the data we shall afterwards supply when studying in one view the numerical proportions of the diseases which ravaged the contending armies:—

Battle of the Alma, September 20, 1854.—Detail of Allied and Russian Forces.

—	FRENCH			ENGLISH		TURKS		RUSSIANS	
	Officers	Troops	Ordnance	Officers and Troops	Ordnance	Officers and Troops	Ordnance	Officers and Troops	Ordnance
Staff . .	35	100	—	—	—	—	—	—	—
Infantry .	853	24,250	—	18,000	—	—	—	50,000	—
Cavalry .	7	203	—	1,200	—	—	—	6,000	—
Artillery .	103	2,676	68	1,000	50	7,000	12	3,000	140
Engineers .	43	866	—	480	—	—	—	—	—
Med. Service .	41	—	—	} 800	—	—	—	1,000	—
Military Train	105	1,046	—						
Totals .	1,187	29,141	68	21,480	50	7,000	12	60,000	140

TOTAL OF ALLIES.

			Artillery				Artillery.
French, .	30,328	...	68	Russians, 60,000	...	140	pieces.
English, .	21,480	...	50				
Turks, .	7,000	...	12				
			58,000				130 pieces.

It is to be observed, that the foregoing numbers, given by M. Chenu for the British forces on the day of the battle of the Alma, do not correspond precisely with the official records. There appear to be no documents in the Adjutant-General's offices which will give with absolute certainty the numbers of the troops engaged; but a letter to the late Duke of Newcastle showed the strength of the various arms of the service to have been as follows:—

1,100 Cavalry.
3,100 Artillery and Engineers.
22,600 Infantry.

Total, 26,800—(*Information from Horse Guards, Oct. 24th, 1866.*)

Battle of the Alma, September 20, 1854.—Killed, Wounded, or Prisoners.

—	FRENCH ARMY			ENGLISH ARMY			RUSSIAN ARMY		
	Killed	Wounded	Disappeared	Killed	Wounded	Disappeared	Killed	Wounded	Disappeared
General Officers . . .	—	2	—	—	1	—	—	5	1
Officers	3	59	1	26	92	—	45	96	—
Non-commis. Officers and soldiers	138	1,136	2	322	1,539	—	1,762	2,720	1,007
Totals	1,341			1,970			5,636		

Losses of 17th Oct., 1854.—Bombardment by Allied Fleets, and Land Attack.

FRENCH ARMY AND FLEET
Killed Wounded
61 401

ENGLISH ARMY AND FLEET
Killed Wounded
304 266

TURKISH ARMY AND FLEET
Killed Wounded
? ?

RUSSIAN ARMY
Killed and Wounded
500

The strength of the English land forces on the 31st October was 35,445.—(*Horse Guards information.*)

Battle of Balaklava.

	Killed	Wounded
French troops engaged, . . .	...	...
English troops engaged, . . .	...	...
(Cavalry, 400 killed and wounded out of 600.)		
Turkish troops engaged, . . .	...	...
Russian troops engaged, . . .	2,500, with 40 pieces of cannon; killed and wounded, 500.	

The following is the state of the British army in the Crimea on the 24th October, 1854, the day before the action at Balaklava.—
(*Horse Guards information.*)

	Cavalry	Artillery & Engineers	Infantry
Under arms, ...	1,916	3,125	18,639
Detached on command, batmen, } or otherwise employed, }	421	217	3,816
Sick, { Present, ...	234	48	1,001
{ Absent, ...	307	130	5,526
	2,878	3,520	28,982

Total officers and men, 24th Oct., 1854, 35,380

Battle of Inkermann, November 5, 1854.

French troops, .	41,019
English troops, .	14,000
Turkish troops, .	

In the month of July, 1855, the hospital requirements of the French army in the East reached the following astounding figures. Twenty-three secondary hospital establishments, radiating in and around Constantinople, afforded accommodation for 19,182 beds, while in the Crimea, at the end of July, there remained 6,816 cases, while the fleet comprised 2,803 sick;

Total, 28,801

After the 8th September the hospital requirements in the Crimea alone rose to over 20,000 beds for sick and wounded inclusive. The situation of the army on the 15th of August, the eve of the battle of the Tchernaya, showed, of a total force of 119,113, the large number of 27,588 ineffective from various causes, and so gave a burden and embarrassment to the army as well as a loss.

Battle of the Tchernaya.

French army engaged, 30,000.

Russian army engaged, 70,00.

Killed	Wounded	Missing	Killed	Wounded	Wounded carried off	Prisoners
291	1,227	46	3,320	1,814	6,000	498

On the 3rd September the advanced works of the French army were within 25 to 30 metres of the Malakoff, 40 metres of the Little Redan, 50 metres of the Flagstaff Battery ; the advanced works of

the English army were within 200 metres of the Grand Redan. The daily losses are considerable. The French have about 150 men killed and wounded every twenty-four hours; the English, in proportion; the Russians, obliged to retain within their walls a force sufficient to resist an assault, suffer, from the converging fire, losses which are estimated at 1,500 to 2,000 per day.

On the 5th September the fire of all the batteries is opened along the whole line; the French count 635 pieces of ordnance, the English 179, the Russians about double. The sublime grandeur of the effect thus produced who can forget that was there to share!

The French losses during the bombardment, 1st to 8th Sept., are thus reported:—

Killed	Wounded	Missed
204	1,325	4

The Russian losses are thus given:—

	Killed and Wounded
17th August, ...	1,500
17th to 21st August, ...	5,800
22nd August to 5th September, ...	8,550
5th September to 8th, ...	3,968

The effective troops on the memorable 8th September are thus enumerated:—

French, ...	126,705
English, total forces. . . .	55,810
Sardinians, ...	
Total, ...	190,420

Losses.

FRENCH			ENGLISH		
Killed	Wounded	Missing	Killed	Wounded	Missing
1,634	4,783	1,416	385	1,910	176
SARDINIANS			RUSSIANS		
Killed	Wounded	Missing	Killed	Wounded	Prisoners
4	36	—	2,684	7,290	1,763
Total killed, ...			4,707		
,, wounded, ...			13,749		

On the 15th November took place the terrific explosion of the powder magazine, and ammunition of all kinds stored in the park of artillery near the Mill of Inkermann. These magazines contained 50,000 kilogrammes of powder, 600,000 cartridges, 4,000 large projectiles and fusees, the fragments of which were carried to a

distance of more than 1,200 metres. No sight of more terrific grandeur can be well conceived than that offered by this explosion. After the first explosive movement upwards a column of smoke, heterogeneous *debris* and projectiles of most colossal proportions, apparently some 200 feet in diameter, continued to rise with a steady and majestic motion until it seemed to bear up the heavens upon its summit. High in mid-air shot, shell, and projectiles of all kinds were thrown off from the centre, starring the mass with the fire of live shells successively exploding, and seaming the sky in gigantic spray-like arches with the shooting fires of thousands of rockets, fusees, &c.

The losses by this explosion were:—Killed, 20; wounded, 114; missing, 18; besides many animals, as horses, cattle, &c., killed and fearfully maimed.

The following table (page 162) will show, for each month of the years over which the eastern campaign was spread, the casualties by arms of war and by disease for the French army.

The strength of the British forces at Inkerman, as here given by M. Chenu, is certainly incorrect, and greatly over-estimates the actual numbers of troops engaged on that day so memorable in the annals of the British army. It was stated in Lord Raglan's despatch that the numbers actually engaged at the battle of Inkerman scarcely exceeded 8,000 men. The following is the return of the strength of the army on the 4th Nov., 1854:—

	Cavalry	Artillery, Sappers and Miners	Infantry
Under arms, ...	1,417	3,154	17,436
Detached on command, batmen, } or otherwise employed, }	449	260	4,355
Sick, { Present, ...	149	50	1,131
{ Absent, ...	512	130	5,104
	2,527	3,594	28,024
Officers, ...	107	147	708
Total officers and men, Nov. 4th, 1854,	35,107		

The brigade of Highlanders, upwards of 2,000 men, was at Balaklava; the 3rd division, of 3,400 men, occupied the heights in rear of the trenches, a sortie from the town being expected; and about 3,600 were in the trenches; these deducted from 17,436,

the infantry under arms, leave 8,436, nearly agreeing with the number given in Lord Raglan's despatch.*

Russian Forces.

1. Attack on Inkermann, under Gen. Damenberg, 50,000, with numerous artillery.
2. Attack on Balaklava, under Gen. Liprandi, 20,000, with cavalry and artillery.
3. Sortie on the besiegers, under Gen. Timofieff, 5,000, with artillery.

Details of Losses at Inkermann.

	FRENCH			ENGLISH			RUSSIANS		
	Killed	Wounded	Missing	Killed	Wounded	Missing	Killed	Wounded	Prisoners
General Officers . . .	—	2	—	3	5	—	1	—	—
Officers	26	96	1	40	97	—	61	206	—
Non-commis. Officers and Soldiers	203	1,453	69	486	2,184	—	6,000	9,200	267
Totals, ...	1,850			2,815			15,468		

Losses by the French Troops.—Attack on Mamelon Vert, June 7, 1855.

Killed	Wounded	Missing
697	4,363	303

Losses 18th June, 1855.

FRENCH ARMY			ENGLISH ARMY			RUSSIAN ARMY	
Killed	Wounded	Missing	Killed	Wounded	Missing	Killed	Wounded
1,370	1,765	416				743	4,979

Strength of the British Army at various periods during the siege of Sebastopol:—

	Cavalry.	Artillery & Engineers.	Infantry.
Under arms, ...	1,274	2,780	18,612
Detached on command, batmen, } or otherwise employed, }	593	278	4,741
Sick, { Present, ...	266	29	2,110
{ Absent, ...	509	598	6,338
	2,642	3,685	31,801
Total officers and men, 27th Nov., 1864,	38,128		

* Medals of the British Army, by Thomas Carter, Adjutant-General's Office. Vol. i., p. 45.

Under arms, ...	1,197	3,132	17,882
Detached on command, batmen, } or otherwise employed, }	548	306	5,069
Sick, {Present, ...	193	564	4,821
{Absent, ...	576	673	9,174
	2,514	4,675	36,946

Total officers and men, 11th Jan., 1855, 44,135

Under arms, ...	1,264	3,804	18,931
Detached on command, batmen, } or otherwise employed, }	409	243	3,869
Sick, {Present, ...	161	259	3,422
{Absent, ...	322	673	9,746
	2,156	4,979	35,968

Total officers and men, 1st April, 1855, 43,103

Under arms, ...	2,822	5,057	25,651
Detached on command, batmen, } or otherwise employed, }	492	292	3,646
Sick, {Present, ...	277	654	2,751
{Absent, ...	97	396	6,592
	3,688	6,399	38,640

Total officers and men, 18th June, 1855, 48,727

Under arms, ...	3,643	5,847	29,913
Detached on command, batmen, } or otherwise employed. }	757	306	3,896
Sick, {Present, ...	796	739	3,261
{Absent, ...	370	559	5,723
	5,566	7,450	42,793

Total officers and men, 7th Sept., 1855, 55,810

Das Chloroform—Eine Zusammenstellung der bisher über dasselbe gemachten wichtigsten Erfahrungen und Beobachtungen vorzüglich in physiologischer und medizinischer Beziehung. Von DR. FRIEDRICH SABARTH. Würzburg: 1866. Pp. 276.

Chloroform—A Collection of the Most Important Experiments and Observations which have hitherto been Made on this Substance, chiefly in a Physiological and Medical Point of View. By DR. SABARTH.

THE plan of the work before us is sufficiently explained in its title. The object of the author is not so much to record his own observations and conclusions, as to collect from various scattered sources and to arrange systematically all that has been written by others on the medicinal uses of chloroform. This design he has accomplished with the laborious and painstaking industry which is so characteristic of German writers.

The earlier sections are devoted to the chemistry of chloroform, its production, chemical and physical properties, signs of purity, &c.; after which is an account of the different modes which have been employed from the earliest times to the present for the production of anesthesia. We then come to a long chapter containing an account of the experiments which have been made on animals in order to ascertain the physiological action of chloroform inhalation, and to determine how it causes death. The results arrived at by different experimenters are somewhat discrepant. We choose for quotation those of the French Commission appointed by the Society of Emulation, as being the fruit of numerous and carefully made observations. 1. The rapidity with which chloroform acts varies with the concentration in which its vapour is inhaled; the phenomena which it produces develop themselves always in the same succession, and with the same characters. 2. Chloroform abolishes the excito-motor influence of the nervous centres and the sensibility and motility of the spinal nerves; the excitability of the spinal cord and the power of the nerves to excite motion under the stimulus of electricity still persist. 3. Chloroform has a special affinity for the nerve centres, in the substance of which it accumulates during inhalation; here it is found after death in greater quantity than in other parts. 4. In chloroformed animals the movements of respiration cease before those of the heart; the heart is the part of the

body which dies last. 5. The animals die if left to themselves after the cessation of respiration. 6. Chloroform is very quickly removed from the body; the lungs are the organs which are most active in this elimination; the skin helps but little. 7. Electricity exhausts quickly the irritability of the nerves in animals who have arrived at the last stage of chloroform poisoning. 8. Death, when it arises from chloroform inhalation, is the consequence of an annihilation (*vernichtung*) of the influence of the nervous system, and not the consequence of asphyxia or of paralysis of the heart. 9. The dilution of the vapour of chloroform with a large and constant proportion of atmospheric air can, if not quite remove the danger, at least greatly postpone and diminish it.

With these conclusions the results obtained by Weber for the most part agree; he, however, objects to the seventh and eighth conclusions of the Commission. He can find no evidence to show that in a chloroformed animal the nerves are exhausted by electricity more quickly than in an animal not under the influence of the anesthetic, and he argues that though the apnea and the stoppage of the heart are caused by the abolition of nervous influence, yet that they are the immediate cause of death, as is shown by the recovery of the animal if the action of the heart be kept up by artificial respiration till the poison is eliminated. No mention is made in this chapter of the recent experiments of the Chloroform Committee of the Medico-Chirurgical Society. This is to be regretted, as they were better conducted and more conclusive than any of those which have been recorded by Dr. Sabarth.

In the next chapter we find details of 119 cases in which death is said to have been caused by chloroform inhalation. After an accurate examination of these the author thinks that in 48 only can, with certainty or high probability, the fatal event be attributed to the anesthetic. Of these 48 deaths 36 were caused by asphyxia, 11 by syncope, and 1 by spinal apoplexy. Although asphyxia is thus shown to produce the greatest number of deaths, yet syncopal symptoms are more frequently met with; they are, however, more easily combatted than is asphyxia.

Dr. Sabarth finds, like Dr. Charles Kidd, that fatal effects are more frequently produced when chloroform is administered for small operations than when it is given for amputations or other operations of the first magnitude. With regard to the stage of inhalation at which death most frequently takes place, he gives the following numbers, taken from the works of Snow, Scontetten, and Kidd.

Of 121 cases, death occurred in 54 during the administration of the chloroform and before the commencement of the operation; in 42 during the performance of the operation; and in 25 after the completion of the operation, either immediately or after an interval. Hence it appears that the early stage of inhalation, or that of excitement, is the most dangerous. Of 133 fatal cases recorded by the same authors, in which the sex is noted, 90 occurred in men, and 43 in women.

After death from chloroform the *post mortem* appearances are not at all characteristic. A dark and fluid condition of the blood, emptiness, pallor and flaccidity of the heart, and gas bubbles in the veins, are among the most important of those which have been recorded. On the whole subject of death from chloroform the author thus concludes:—"When compared to the innumerable multitude of those to whom chloroform has been administered and to whom it is yearly and daily administered, the small number of cases in which death can be with certainty or probability attributed to this agent holds but a very small proportion. We must further consider that of the hundreds of thousands who have been chloroformed certainly many more would have died of anxiety, disquietude, or fear before the operation—of pain or agitation during its performance, or of its consequences, if chloroform had not been employed, than have succumbed to the poisonous effects of the latter. So that, considered in this light, the advantages of chloroform greatly preponderate over its disadvantages."—P. 156.

The precautions to be used in giving chloroform by inhalation, and the means to be employed when dangerous symptoms arise, are treated of at considerable length. These matters have, however, recently been brought under the notice of our readers in the review of Dr. Sansom's work on chloroform.^a We, therefore, think it unnecessary again to go into the subject, more particularly as in the abstract from Dr. Sansom's book, to which we refer, more information will be found in a few sentences than can be extracted from the thirty pages which Dr. Sabarth devotes to the administration of chloroform, and the means of restoring suspended animation. We may mention, however, that the latter author prefers to give the chloroform on a folded towel or handkerchief, believing that with proper precaution this method is just as safe, and less terrifying to the patient, than that in which an elaborate inhaler is

^a *Dub. Quart. Jour.*, May, 1866, pp. 354; *et seq.*

used. Some very striking cases are recorded in which, after all the ordinary means had failed to restore the suspended respiratory movements, galvanism succeeded, one pole of the battery being placed over the phrenic nerve in the neck, the other over the origin of the diaphragm. In one successful case twenty minutes had elapsed from the commencement of the asphyxia before the pulse and respiration were restored.

In the next chapter we find the opinions of authors on the value of chloroform in the treatment of medical diseases. To enumerate all the maladies in which this drug is said to have been employed with the most perfect success, would be to give the entire nosological list, from hiccup to tetanus and hydrophobia. We must confess ourselves somewhat sceptical as to its applicability or value in some of the cases in which it appears to have been largely used. Thus, first on the list stands pneumonia, a disease which seems to lend itself with equal complaisance to swell the tables of successes of every mode of treatment from calomel and blood-letting to homeopathy; and we find the chloroformist as fortunate as Sangrado or Hahnemann. So, we read, that of 193 cases treated by chloroform inhalation by Drs. Wachern, Baumgärtner, Helbing and Schmidt, only nine died; and that Dr. Varrentrap, of Frankfort, treated twenty-three cases (nineteen exclusively) by chloroform inhalation, and of these only one died. These results are very encouraging; but in the absence of any details as to the age of the patients, the amount of lung tissue involved, the condition of other organs, and the various other circumstances which, much more than the treatment, make the difference for the patient between life and death, we must confess our unwillingness to repeat the experiments of our German brethren.

In spasmodic affections of the respiratory organs, unaccompanied by structural change, we can readily believe in the advantages to be derived from chloroform. And under this head we find cases of spasmodic asthma, hooping cough, hysterical cough, laryngismus, stridulus, and hiccup, reported as yielding readily to chloroform inhalation.

In the paragraph on hepatic colic and gall-stones, we find the internal administration of chloroform in solution in alcohol strongly recommended on the authority of M. Bouchut, but are somewhat surprised not to meet with one word advising its inhalation in these circumstances; a mode of exhibition of the drug which would

manifestly be much more efficacious than its internal administration, notwithstanding its vaunted power of dissolving cholesterine.

In these days when cures for cholera abound, our readers will not be surprised to learn that chloroform possesses anti-choleraic virtues unsuspected by many. Not only has it been given with success internally, combined with opium and other drugs, but also by inhalation, and we are directed to keep the patient under its influence until all symptoms indicating danger have passed away.

Fifteen cases of intermittent fever were cured by Bonafont by inhalation of ether or chloroform. The inhalation was employed six hours before the expected attack; if the latter was once escaped completely, further treatment was unnecessary; the cures were all permanent. The patients preferred this mode of treatment to that by sulphate of quinine. In Spain, Poblacion and others have found the internal administration of chloroform useful in intermittents.

In cases of strychnine poisoning, chloroform inhalation has proved itself of great service. The following example is interesting:—A man, 40 years of age, whose habits were somewhat irregular, drank in mistake for morphia, the contents of a bottle which held a solution of strychnia. In twenty minutes the following symptoms manifested themselves:—Rigidity of all the muscles, contraction of the muscles of the back, and of the upper and lower extremities, retraction of the head, difficulty of speech, oppression about the chest, profuse perspiration. The means usually resorted to were fruitlessly employed. It seemed as if the patient would succumb to the severity of the spasms, and death appeared inevitable unless speedy assistance were afforded. Dr. Mannson, at this juncture, administered chloroform. Two grammes were poured on a pocket-handkerchief, which was held so that the patient could conveniently inhale the vapour. He was then in the sitting position supported by many assistants, but soon desired to be laid in bed. The inhalation was continued off and on for many hours, the patient himself holding the handkerchief to his nose in order, as he said, to cut short the cramps. These gradually disappeared, and after two days the patient was perfectly well and able to resume his usual occupation.

In a physiological digression the author explains the presence of sugar in the urine, after the inhalation of ether or chloroform, by the impairment of respiration, which interferes with the supposed combustion, in the lungs, of the hepatic sugar. That the diabetes is not due to a reflex influence exerted through the pneumogastric nerves on the medulla oblongata, is shown by dividing the vagi in

the neck of a rabbit, previous to the commencement of the inhalation, after which sugar appears in the urine just as if no operation had been performed. The diminution of animal heat, which Duméril and Demarquay have shown to be consequent on the inhalation of ether or chloroform, is independent of respiratory disturbances, and is supposed by the author to be caused by a specific action exerted by the anesthetic on the nervous system.

This chapter terminates by a consideration of the combined influence on the body of chloroform and morphia. A combination of these for internal administration may be prepared as follows (Bernatzik):—One part, by weight, of morphia is dissolved in two parts of rectified wine vinegar and twenty parts of rectified spirit of wine, and the solution, when cold, is mixed with eighty parts of chloroform—one drop contains the $\frac{1}{80}$ part of a grain of morphia. Dose for a child, two to fifteen drops; for an adult, thirty to forty drops. It is said to give relief in most painful affections much more quickly and certainly than morphia alone, and to leave none of the unpleasant after effects of opium. The subcutaneous injection of morphia, during chloroform narcosis, is strongly advocated in all those cases where it is desirable to maintain the state of unconsciousness for a lengthened period.

In the following chapter, some of the more important surgical applications of chloroform are considered. In operations about the mouth the patient is often deprived of the advantages of anesthesia through fear that suffocation might ensue from passage of blood into the larynx. The general opinion appears to be that such fear is exaggerated. Chassaignac advises us to watch for the moment at which, when the patient is recovering from the influence of the chloroform, consciousness awakes, while sensation is still in abeyance. If at this moment we make our incisions the patient feels no pain, and at the same time is sufficiently his own master to reject the blood which accumulates in his mouth, and to prevent its entrance into the air passages.

On the importance of chloroform in operations on children, in facilitating the taxis of hernia, and the reduction of dislocations, it is unnecessary to speak. The uses of chloroform, applied externally, for the production of local anesthesia, the cure of itch, &c.; and as a solvent for different substances, specially gutta percha and camphor, are briefly noticed at the end of this chapter.

With regard to the administration of chloroform in operations on the eye, the generally received opinion is that it is applicable in

almost all cases except those in which the globe is largely opened, as in flap extraction of cataract. Here, the danger of prolapse of the iris with loss of vitreous, and perhaps separation of the retina and choroid, is so great, should vomiting occur, as to render the employment of chloroform extremely hazardous. We find, however, Jacobson advocating its use in cases of flap extraction as being perfectly free from danger, and as greatly facilitating the performance of the different steps of the operation. He says that by suitable regulation of the patient's diet, previous to the operation, and by the use only of pure chloroform, the occurrence of vomiting may be, in most instances, entirely prevented, and in those few cases where it does occur it may, by proper care on the part of the operator and his assistants, be rendered perfectly harmless for the eye. He supports his opinions by the results of 150 cases of flap extraction, in which chloroform was employed without ill effects.

The chapter on the uses of chloroform in midwifery practice is quoted almost entirely from Scanzoni. This writer disapproves of the administration of chloroform in perfectly natural labour, but in those cases in which it is necessary to moderate uterine action, and in most obstetric operations, he strongly advocates its employment. Of some novelty is his recommendation of chloroform as a means of warding off a threatened abortion or miscarriage. He relates a case in which uterine action had continued for some time, and the os was dilated to the size of a silver groschen. After inhalation had been maintained for half an hour the pains ceased entirely, the os quickly closed, and the patient went to her full time without further accident. She was a primipara; the threatenings of abortion had come on after a severe mental emotion; the period of her pregnancy is not mentioned. Scanzoni thinks also that in eclampsia chloroform is of service, not only in diminishing the severity and frequency of the fits, but also in helping to preserve the life of the child, which is generally lost, not by uremic poisoning of its blood, but by the interruption of the nutritive processes between the mother and child, produced by the spasmodic contraction of the uterine walls during the attacks. This spasm, the author thinks, can be mitigated by the use of chloroform.

The last chapter contains the history of the *post mortem* appearances of persons dead from chloroform inhalation. As has been already stated, with the exception of flaccidity and bloodlessness of the heart, the appearances are not characteristic of the cause of death.

On the whole, the work before us leaves, after its perusal, a somewhat unsatisfactory impression on the mind. Out of the mass of heterogeneous, and often contradictory, opinions contained in it, it is difficult to choose the good and true, and to reject that which is fanciful, or founded on insufficient observation. We think that those who wish to learn what is really known on the subject of chloroform will do better by consulting a book written by a single author, than one made up of cuttings from the writings of many. At the same time we would acknowledge the value of the present work as a contribution to medical bibliography.

A Practical and Theoretical Treatise on the Diseases of the Skin.

By GEORGE NAYLER, F.R.C.S., Assistant-Surgeon to the Hospital for Diseases of the Skin, Bridge-street, Blackfriars
8vo. London: Churchill & Sons. 1866.

THIS volume consists of 292 pages of large type, on toned paper; and, besides plates, to which we shall afterwards advert, it comprises twenty-two chapters, of which we shall endeavour to give a short analytical outline.

Chapter I.—“On the Anatomy and Physiology of the Skin”—covers 21 pages, with an amount of matter which, in our opinion, is not at all necessary or desirable in a book of this kind. Any one who studies the subject of cutaneous medicine is presumed to know something of the anatomy and physiology of the skin, or at least to know that in anatomical and physiological works is the proper place in which to look for such information. If a man write a book on diseases of the heart, or of the liver, or of the kidneys, he is not expected to preface his treatise with an anatomical primer taken from the received writers of our own day.

Chapter II.—“On Psoriasis and Lepra”—treats of substantially one affection under the two names; and the mode of arrangement adopted by Mr. Nayler in each chapter may be well illustrated here. We first are told of the varieties of psoriasis, and then of its general characters and situation; its hereditary tendency and causes are next treated of; and here Mr. Nayler observes (p. 24):—“I am able to confirm the statement of Neligan, that psoriasis sometimes appears in the collateral branches of a family, while the immediate descendants are free from the disease. It is more common, however, to find it invade in the direct line.” Mr. Nayler now proceeds

to discuss the relative frequency of psoriasis in the two sexes; enters on the question of diagnosis; speaks of *psoriasis annulata*; syphilitic psoriasis; the absence of scales in syphilitic psoriasis; of *ps. prominens*; of the question of prognosis; and of treatment, particularly as regards salines, arsenic, and mercury used internally, and the local use of carbolic and nitric acids, and many other remedies. He recommends a lotion of carbolic acid, consisting of 14 grains of the acid, half an ounce of spirits of wine, and an ounce of glycerine, and a pint of water. Mr. Nayler does not omit to mention the use of baths in this disease; and he adverts to the question of diet; concluding Chapter II. with notices of the various varieties of psoriasis.

Chapter III.—“On Pityriasis”—does not contain anything new, and may, perhaps, be looked on as giving the views of Mr. Startin, Mr. Nayler’s teacher. *P. rubra* and *p. nigricans* are given as having been “noticed by Cazenave, Startin, and others;” “but” he adds, “these are exceedingly rare.” Mr. Nayler maintains the opinion that “*pityriasis versicolor*, or *chloasma*, is of parasitic origin.”

Chapter IV.—“On Ichthyosis”—contains a variety of statistics respecting this disease. Under the head of “Ichthyosis of the Tongue” we read (p. 62):—“Mr. Hulke has recorded a case of ichthyosis of the tongue in a man, a patient at the Middlesex Hospital, who had it upwards of fourteen years. It differed from a corn in its tendency to increase at its margin. The treatment consisted in paring [*sic*] at intervals, as much of the upper part of the growth as could be removed without injury to the tongue.” The condition of the urine in ichthyosis is carefully noted, and tabular statistics of experiments, made by himself, are given by Mr. Nayler. They show that the specific gravity in such cases is low; while “notwithstanding the small rate per cent. of urea, the absolute amount in twenty-four hours is little altered in ichthyosis; and this is somewhat remarkable when we consider that the skin, as a secreting organ, is reduced to the lowest possible limits, in the disease. This fact militates against the usually received theory of the elimination by the skin of urea (p. 65).”

Having disposed of diseases “of the squamous class,” Chapter V. enters on the discussion of “papular eruptions,” which, we are told, comprise *lichen* and *prurigo*; and include under the former, *strophulus*, or gum rash.” Two pages of Chapter V. are occupied in describing *lichen scrofulosus* and *lichen ruber*, of Hebra, to whose article on the subject, in Virchow’s Handbook, the reader is referred for further information.

Chapter VI. treats of prurigo; and Mr. Nayler here maintains that:—"No form of prurigo is in any way dependent on pediculi, although their presence may greatly aggravate the symptoms." The opinion of Alibert, which Mr. Nayler here opposes, and which opinion, he remarks, has been lately revived, is further referred to in a foot note on p. 86, where, admitting the coexistence of pediculi and prurigo, in the lower orders of the people, he observes:—"There is probably no disease of the skin so common in the highest range of society as this variety of prurigo."

Chapter VII. treats of eczema; and, in our view, it is singularly deficient as regards this disease. In a dogmatic manner the disease, which is perhaps the most frequent of all other cutaneous affections, is pronounced *vesicular*; and there is not a word in the entire chapter to indicate that such a thing as controversy regarding the nature and extent of eczema exists. We are quite sure that Hardy and Hebra, and M'Call Anderson, and Fox, and other writers of our own day, will think little of this chapter.

In Chapter VIII.—"On Herpes"—Mr. Nayler maintains that herpes circinnatus (for so he spells the latter word) and tinea tonsdens are one and the same; and he further holds that the affection (h. circinatus) is parasitic. As to its cure, Mr. Nayler mentions as successful the plan adopted by Mr. Coster, Medical Superintendent of the London District School, at Hanwell. It consists of the application of tar and iodine, an ounce of the former to two drachms of the latter; and he gives the mode of its application in a foot note on p. 123.

Chapter IX.—"On Pampholix"—which he regards as synonymous with pemphigus, concludes Mr. Nayler's remarks on vesicular affections; and in Chapter X. he commences on diseases of the pustular class, with porrigo; continuing, in Chapter XI., with impetigo; and concluding, in Chapter XII., with ecthyma and rupia. The porrigo of Mr. Nayler is not the porrigo of Neligan and others (See Dr. Belcher's edition of Neligan, p. 404), but appears to be merely a variety of impetigo. Accordingly, we are not surprised to find him saying, on p. 138, that he has been unable to discover a vegetable parasite in porrigo; but he adds that in numerous cases which he has seen the hair bulbs become flattened, and expand into a broad and thin plain, which terminates in an even edge. He appends microscopic diagrams to illustrate this statement.

Chapter XIII. treats of Lupus; and while it mentions in a passing manner the plastic operations of Mr. Hamilton, of this

city, it wholly omits mention of Jacob's ulcer, which Neligan included under *lupus devorans*.

Chapter XIV.—“On Alopecia”—is better, in most respects, than the corresponding parts of other works on this portion of the subject, because it discusses the question—which it answers in the negative—is alopecia a parasitic disease? and gives microscopic diagrams in illustration.

In Chapter XV.—“On Acne”—Mr. Nayler derives the term *acne* from the Greek α , non, and $\nu\omega\omega$, *radere*, viz., freedom from itching. Without entering into a critical discussion on Greek derivations, we must, with all respect, dispute Mr. Nayler's Greek on this point, and prefer Dr. Belcher's derivation—that usually accepted—as given in his edition of Neligan, p. 173:—“*Acne* is derived from $\alpha\kappa\mu\acute{\eta}$, from its appearance at the full growth and evolution (*acme*) of the system.”

Chapters XVI., on *Sycosis*, and XVII., on *Favus* (the *porrigo* of Neligan) do not call for any particular remark; and we next proceed to Chapter XVIII., on *Scabies* and *Pediculi*. This contains several large representations of the *acarus scabiei*, and of the *pediculus corporis humani*. Mr. Nayler looks on *scabies* as *always* caused by the presence of the *acarus*; and while he recommends the use of the hyposulphite of lime (so common in our military hospitals) for its cure, he seems to think the use of that preparation more modern than it really is. Its use was mentioned in this *Journal* in 1862; and even then it was not new.

Chapter XIX. treats of *Uticaria* and *Erythema*; and Chapter XX. of *Elephantiasis*. Chapter XXI. notices various “morbid growths of the skin”—as, for example—warts, fibrous, sebaceous, and other tumours; and Chapter XXII., which concludes the book, consists of “Remarks on diseases of the Skin following Vaccination.” Mr. Simon, in his papers on vaccination, published in 1857, and presented to both Houses of Parliament, strongly contested the theory of the transmission of syphilis by vaccination. Mr. Nayler, however, believes that cases occur in which there is no room for any other opinion than that syphilis has been transmitted through the medium of vaccination.

There are seven plates prefixed to this volume. Five of them are illustrations of the anatomy and physiology of the skin; and, in fact, appear to have been the pegs for hanging Chapter I. on. The remaining two are microscopic views of parasites.

The classification, or absence thereof, in this book is curious,

and indicates a reaction from the intolerable classification-making of modern writers. Mr. Nayler praises Willan's classes, as every one else does; and, for the most part, he leaves them after him, as do his brother writers. Begging the question, and proving nothing as to what diseases are, respectively, squamous, papular, vesicular, and pustular, he devotes a chapter to each of these groups, and gives one also to each of the remaining diseases "which have their own distinctive characters." According to the modern meaning of the word "Treatise," this book, which is good, so far as it goes, has no proper claim to be called by that comprehensive term. Omitting some diseases altogether, it almost wholly ignores the important labours of other modern writers, except, perhaps, Mr. Startin, and now and then, a man whom no one can afford to ignore—Erasmus Wilson; rarely cites an authority; and quietly omits mention of some prevailing opinions which every well informed practitioner should know something about. The ill-natured critic has some ground for asking—if the drawings and illustrations already noticed had not been in existence previously would this book have been written? If it had been called "A Treatise on the Anatomy and Physiology of the Skin; with Remarks on the Parasitic Nature of Some of its Diseases, and the Treatment of Others"—it would have been well called so.

While thus reflecting on some of the defects of this book, and wondering where the eruption of books on dermatology will end, it is but fair to say that the practical recommendations of Mr. Nayler are sound and useful; the descriptions of disease, for the most part, are clear and concise; and in the important matter of diagnosis his book may be consulted with advantage.

The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy. By MORELL MACKENZIE, M.D., London; M.R.C.P.; Physician to the Hospital for Diseases of the Throat; and Assistant Physician, and Co-lecturer on Physiology at the London Hospital. Second Edition, Revised and Enlarged.

THE volume which has just come to hand is both a surprise and a pleasure. It is not often our pleasing duty to call our professional brethren's attention to a second edition, revised and enlarged, within

so short a space of time. We reviewed the first edition in our February Number of 1865. We then stated, that we felt sure of the success of the book, as it supplied a want. The result has proved the correctness of our opinion.

The arrangement of the volume before us is the same as the first edition; but each chapter is enlarged by the addition of more information, and enriched by many additional well executed engravings.

Mackenzie, as almost every one now does, gives the credit of the Laryngoscope, as it is now used, to Czermak:—

“In the year 1857, in the month of November, Professor Czermak, of Pesth, borrowed from Dr. Türk the little mirrors which that gentleman, in spite of the exhortation of his friends, had thrown aside as useless. In a short time his superior genius, untiring perseverance, and natural dexterity, enabled him to overcome all difficulties. When the dentist’s mirror passed into the hands of Czermak, the examination of the larynx was dependent, so to speak, on the clocks and the barometer; but he soon relieved it from both these troublesome monitors. Artificial light was substituted for the uncertain rays of the sun; the large ophthalmoscope of Ruete, was used for concentrating the luminous rays; the awkward hinge which united the laryngeal mirror to its stem was dispensed with; and the mirrors were made of different sizes. Thus it was that Czermak created the art of Laryngoscopy.”

The second chapter treats of glasses, reflectors, and lamps; of the first, the author prefers the circular forms, except when enlarged tonsils are present; he then prefers oval mirrors.

The reflector may be placed in different positions, either

Opposite one eye (Czermak);

In front of the nose (Bruns);

Or on the forehead (Fournié, Johnson, Mason, &c.).

Of these, the first is the most perfect; the last the easiest in practice.

“Any lamp that gives a bright steady light answers the purpose perfectly well.”

For those who want more, Dr. Mackenzie has a rack-movement laryngoscopic lamp, which admits of perpendicular and horizontal movement. He adds:—“The power of the light may be advantageously increased by one or more lenses placed in front of the flame.” We very much question the advantage of the

increase of light; we have been very glad to diminish the light of our own "moderator" after several examinations; and if we have light enough to see distinctly all that is to be seen, more light only injures the sight of the examiner. The next lamp, however, is a very valuable contrivance:—"It consists of a small metal cylinder, three and a half inches long, and two and a half in diameter. This is closed at one end, and at the other there is a plano-convex lens—the plane surface of which is next the flame. The lens is two inches and a half in diameter, and is about one-third of a sphere. In the upper and under surfaces of the cylinder (opposite each other) are two round apertures." These holes are so placed that the flame is at the focal distance from the lens. At the lower part of the tube are two semicircular arms, which, by means of a screw at the side, can be made to grasp tightly the largest lamp chimney, an ordinary candle, or even the narrow stem of a gas jet. The practitioner can carry this light concentrator in his pocket, and always feel certain of being able to illuminate the fauces.

The third chapter treats of the art of laryngoscopy, and first of reflection. It seems to us very unnecessary dwelling on inversions in laryngoscopy, and quite sufficient for the operator to bear in mind that he is looking into a mirror while he is operating on the real object. In the daily operation of shaving does the operator ever think of inversions when he steers clear of his favourite pimple? Does he not simply fix his eye steadily upon it, and guide his razor with a steady hand in every direction, carefully avoiding its sacred precincts? In exactly the same manner should the laryngoscopist guide his forceps, and never once think of inversions. Let our reader try the experiment, and see if he does not give himself a cut where he thinks how his face is reflected and what parts of it are inverted.

It is unnecessary to enter into the practical descriptions, as every laryngoscopist should provide himself with the work.

Numerous ingenious contrivances are described and delineated in the following chapters.

We would, before concluding our necessarily brief remarks, call our readers' attention to Dr. Mackenzie's most valuable invention, by means of which galvanism can be applied directly to the muscular apparatus which moves the vocal cords:—"By a very simple contrivance the electric current can be applied directly to the vocal cords. The important feature in the laryngeal galvanizer

is, that the current does not pass beyond the handle till the sponge is in contact with the vocal cords. . . . Its employment is indicated in functional aphonia, and in most cases of vocal weakness, where there is no structural disease."

Dr. Mackenzie has published a short work on this subject, entitled *The Treatment of Hoarseness and Loss of Voice by the Application of Galvanism to the Vocal Cords*.

In conclusion, we wish this edition the same success as the first. The importance of the subject to *every medical man* is daily becoming more felt, and though every practitioner cannot become such a proficient as to enable him to operate with facility, yet the most over-worked can in a few days master sufficient to enable him to make a correct diagnosis, and not treat every sore throat with a sponge full of caustic wash inside and a fly blister or iodine outside.

The number of laryngoscopes which have been sold within the last year to medical men in the country parts of Ireland shows with what earnestness Irishmen take advantage of the progress of science; to each of these we would say, get the second edition of Mackenzie.

We are glad to see by an advertisement in the front of the volume that the diseases of the throat which we promised our readers in our review last year, is in the press. If it rivals its predecessor the two volumes will form a complete manual of laryngoscopy.

On Diseases of the Respiratory Passages and Lungs, Sporadic and Epidemic: their Causes, Pathology, Symptoms, and Treatment.

By WALTER GOODYER BARKER, M.B. Lond.; Senior Medical Officer to the Worthing Infirmary; Fellow of the Meteorological Society, &c. London: John Churchill & Sons, New Burlington-street. 1866.

THE object of this book being professedly to bring before the notice of the profession a cause of disease, and especially of lung disease, sporadic and epidemic, which Dr. Barker considers is not sufficiently recognized, viz., atmospheric vicissitudes; and, particularly, as in his preface he claims consideration for his views, the result of fifteen years' close attention, we naturally expected this subject to be largely dwelt upon, and exhaustively handled. We are sorry, however, to say that our anticipations have not been

fulfilled; and we find more written on the symptoms and the treatment of diseases than the cause producing them.

Dr. Barker, apologizing for affixing to his book such a high sounding and comprehensive title, explains that his original intentions were very circumscribed, but that, as he proceeded, he found so close a connexion between *cause* and *effect*, it would have been impossible to have demonstrated the one without giving a description of the other, and then, by a necessary consequence, he was led to speak of the remedies.

To his conclusions, as to the meteorological causes of most, or indeed of all, lung diseases, he was inductively led—1st. By a constant observation of the causes of coryza, not merely the normal atmospheric changes which produce this affection, but also the artificial mode of living, which exposes us to so great fluctuations of temperature. 2nd. By observing that, as a rule, diseases of the respiratory apparatus are ushered in by little or no constitutional disturbance, this being consecutive to, and in accordance with, the local disorder. And 3rd. By the greater frequency of coryza, compared with bronchitis, and this latter with pneumonia, the difference corresponding, as Dr. Barker supposes, with the exposure of each part to the external atmosphere.

Diseases of the lungs Dr. Barker divides into four classes

I. Those which have their origin chiefly or altogether in the immediate contact of the atmosphere upon the diseased part. Under this head he treats of coryza, laryngitis, tracheitis, bronchitis, and pneumonia.

II. That which is produced by the application of cold to the surface of the chest, viz., pleurisy.

III. Epidemic disease, as influenza, hooping-cough, and measles.

IV. That which arises from constitutional causes, either hereditary or engendered, viz., phthisis.

In Chapters III. and IV. laryngitis and tracheitis are considered under three heads, according as inflammation attacks the epithelial lining of the larynx and trachea, the basement membrane, and the sub-mucous areolar tissue. Under this last comes croup. Dr. Barker's views of this disease are thus expressed:—"There is no reason to suppose that there is anything specific in the nature of this disease, or that it is limited to the trachea, but rather that the formation of a false membrane is owing entirely to the severity of the inflammation by which plastic lymph is thrown out. This lymph is allowed to accumulate, on account of the feeble expulsive

power of infantile life, and, therefore, becomes moulded to the trachea." The small size of the larynx, and the undeveloped nares of the infant, so that the air cannot be sufficiently altered as regards temperature and moisture, before coming in contact with the mucous membrane of the trachea, are additional reasons why it is so often a disease of childhood.

With respect to the particular condition of the atmosphere under which the disease is lighted up, Dr. Barker's inquiries have almost invariably enabled him to trace its origin to exposure to a cold dry air. The few cases which had occurred in his own practice had always taken place in the Spring of the year, and with the prevalence of an easterly or north-easterly wind; while he has never known a case to occur when the wind blew from the south, south-west, or west.

Passing by an uninteresting chapter on bronchitis, acute and chronic, we come to one on asthma, in which we certainly expected to see the subject of the connexion of this disease with atmospheric phenomena fully discussed, but, beyond a casual one, no mention is made of the atmospheric vicissitudes being its cause; and knowing the great benefit asthmatic patients derive from change of air from the sea-side to the interior, from the country to town, and even to particular parts of town, we are sadly disappointed to find the subject thus summarily dismissed:—"With respect to the atmosphere and locality best suited to the asthmatic, there is so much variety of opinion, and so much caprice in individual cases, that no rule can be laid down."

As neither time nor inclination permits us to give a full synopsis of this work, we intend merely touching upon a few additional subjects, from which the author's views can best be gathered.

In the chapter on pneumonia, Dr. Barker writes:—"Bronchitis and pneumonia, viewed as to the cause, may be considered as the one disease, differing only in degree." Learning that such is his opinion on this all-important affection of the lung, we are not surprised to read that, in the first stage, of Laennec, he treats his patients, as he does a case of acute bronchitis, by bleeding, active purgatives, antimony, and bringing him under the influence of mercury. As this line of treatment is so directly opposed to that now almost universally recognized, especially in the asthenic type of disease so prevalent at present, we can easily understand how beneficial, if not absolutely necessary, the tonic treatment he advises in the third stage will prove.

The most interesting part is that in which Dr. Barker treats of epidemic disease, and penetrates, in particular, the mysteries of influenza. The views hitherto entertained on the subject of its cause he shows to be erroneous, and unsupported by observation. Influenza was attributed at first, in 1633, to a dense fog, by Paulini; to thick sulphurous vapours, by Læu, in 1729; to electric and telluric phenomena, in 1732-37; some combinations of selenium, being assigned as its cause, by Dr. Prout; and by Weber a negatively electrical state of the atmosphere. In 1847, Dr. Peacock considered it due to a poison of a telluric or atmospheric character. Later still, ozone was thought its cause; and, lastly, Dr. Barker advances atmospheric vicissitudes. "I approach," he writes, "this subject with some diffidence, because the opinions that will be advanced are not only opposed to those of the wisest and best of our profession, but, so far as I have been able to discover, of every medical man who has written on the subject." Its nature, he considers, may be thus briefly expressed:—"Influenza is coryza associated with inflammation of the fauces, pharynx, larynx, trachea, and upper bronchi; and, in more severe cases, of their minute ramifications;—and if there be further extension of it, to the air sacs, we say it is complicated with pneumonia." And further on he writes:—"Influenza has no essential character whatever."

After some eight pages devoted to proving the causes of the severe and terribly fatal results of influenza to be the same as those which intensify the other scourges of mankind, viz., cholera, typhus, scarlatina, &c., he asserts, without, we think, anything like conclusive reasoning, that the *essential cause* of this disease is exactly the same as that of coryza, namely, the sudden fluctuation of the temperature of the atmosphere, and especially the cold of the north and north-east winds, and differs from coryza alone in degree—as the cause is an exaggerated one, the effects, therefore, are exaggerated.

Hooping-cough is alike referred to the same source; but, as if Dr. Barker had not ridden his hobby enough, he astonishes us by classifying *measles* with influenza and hooping-cough. He says:—"The exciting cause of measles is undoubtedly the same as of *other lung diseases*!" The only reason we can see brought forward for this novel and startling theory is the circumstance of the respiratory organs being invariably affected in each.

We have now come to the close of our review, already too long, and without further comment leave the worth and usefulness of such a work to the individual judgment of the reader.

Cancer: A New Method of Treatment. By W. H. BROADBENT, M.D., London; Assistant-Physician to St. Mary's Hospital; and Lecturer on Physiology at the St. Mary's Hospital Medical School. London: John Churchill & Sons.

"He who attempts to make others believe in means which he himself despises, is a charlatan; he who makes use of more means than he knows to be necessary, is a quack; and he who ascribes to those means a greater efficiency than his own experience warrants, is an impostor."—LAVATER. *Aphorisms on Man.*

WE wish to draw attention, by a few lines, to Mr. Broadbent's new treatment of cancer. Few who have closely studied the reagents which affect the cancer cell will have escaped the momentary impression that in acetic acid there was a remedy for the disease, if it could be brought into contact with all the component cells of a cancerous tumour. This preliminary difficulty Mr. Broadbent thinks he has surmounted by a process of hypodermic injection. We shall let him speak for himself:—

"The hypodermic syringe is now in the hands of every physician, and it seemed to me that by means of this instrument, some fluid might be injected into the tumour which might so far alter its structure, and modify its nutrition, as to retard or arrest its growth. Various substances presented themselves to my notice, and acetic acid was selected for the following reasons:—

"1. This acid does not coagulate albumen, and might therefore be expected to diffuse itself through the tumour. The effect would thus not be limited to and concentrated in the point injected.

"2. If it entered the circulation it could do no harm in any way, either by acting as a poison or by inducing embolism.

"3. Acetic acid rapidly dissolves the walls and modifies the nuclei of cells on the microscopic slide, and might be expected to do this when the cells were *in situ*.

"4. It had been applied with advantage to open cancer and to cancerous ulcerations.

"The experiment was made, and it was found that acetic acid, though in healthy tissues it causes very severe smarting and burning, unless very strong, gives little pain when thrown into malignant structure. On the other hand it acts energetically on cancer, but has comparatively little effect on normal structure."

The cases of this method given are but four in number, and their

results are but moderately satisfactory. Other cases have been recorded in the journals, and in time a sufficient number of experiences on the matter will be brought together (perhaps in a future pamphlet by Mr. Broadbent) to enable the profession to form a judgment of this method. While we refrain from expressing any sanguine hope as to the value of this particular treatment, we hail all such suggestions as steps towards the ultimate discovery of a method, or methods, to check the growth of this most fearful disease; especially where, as in the present instance, they are free from all suspicion of the "charlatanism, quackery, and imposture" which Mr. Broadbent deprecates in his quotation from Lavater. Mr. Broadbent has given a stimulus to other observers, and we are aware of various fresh efforts which are being made to solve the important problem of the curability of cancer. Of the results of these investigations it will be our duty to keep our readers duly informed from time to time.

A Treatise on the Practice of Medicine. By GEO. B. WOOD, M.D., LL.D., &c. 6th Ed. 2 Vols. Philadelphia: Lippincott & Co. 1866.

WHEN a book has reached the sixth edition what more need be said about it. We have examined this one carefully, and find it brought fully up to the level of the day, and in every respect fitted to maintain the high position already attained by this favourite class book.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.^a

DR. R. W. SMITH, President.

Popliteal Aneurism.—DR. FLEMING said that the case which he begged to lay before the society was one of popliteal aneurism, in which the treatment by compression was apparently advancing satisfactorily, although slowly, when, suddenly, the aneurism became diffused, and it was decided to tie the femoral artery. Suppuration of the sac subsequently supervened, and violent hemorrhage having taken place into it, amputation of the thigh was performed.

The subject of the case was a man aged between thirty and thirty-five years. He had been a guard on one of the Irish railways, and about twelve months back, whilst engaged at his usual duty, forcibly pushed his right foot against the door of a third class carriage in order to shut it, when he felt a most painful sensation in the ham and on the outside of the corresponding thigh and leg, extending to the foot,

Although suffering much pain he continued for two or three days at his employment, when, at the end of that time, he perceived a small tumour in the popliteal space about the size of a bean. This tumour gradually increased, yet he continued at his work for the succeeding six months, at the end of which period he was obliged to apply for relief, and was admitted into the Richmond Hospital in the last week of June. All the essential features of aneurism of the popliteal artery were present. The tumour was about the size of a large orange, and occupied the inferior angle of the popliteal space. It was uniformly smooth on its surface, had a strong expansive pulsation, and a distinct systolic bruit. These signs were completely controlled by pressure on the femoral artery at the pubis; and the sac of the aneurism, moreover, was completely emptied.

^a These reports are furnished by the Secretary to the Society.

rapidly refilling on the removal of the pressure. The man was tall in stature, had an anemic look, and from his history was decidedly of a rheumatic habit. During his stay in hospital he had repeated attacks indicative of this diathesis at irregular intervals, which necessarily interfered with the treatment of the aneurism. The sounds of the heart were natural; its action, however, was feeble and unsteady, and he had precordial uneasiness, with occasional faintness, almost amounting to syncope.

Shortly after his admission into hospital he suffered considerable abdominal uneasiness, often specially referred to the left lumbar region, and accompanied by symptoms strongly simulating the passage of a renal calculus, and materially influencing the condition of the urine from the large amount of lithates present. These symptoms yielded to ordinary remedies; and on their subsidence the treatment of the aneurism by compression was carried out in the most extended application of that term. Throughout it there was not the slightest indication of œdema of the limb, and the pulsation of the tibial artery was perfectly ascertainable. There were occasional interruptions to the compression from local annoyances unnecessary to particularize; when, in the beginning of September, a most obvious change had taken place in the aneurism. It was diminished in size; it was solid in consistence, and its pulsation was extremely feeble; in fact, there was every reason to anticipate its ultimate cure; and my colleague Professor Smith, entertained the opinion that a cure would be effected in the same manner as in a case lately under his care, where the remnant of the aneurismal sac permitted of the passage of the blood through it, a small pulsating tumour being persistent.

About this time it became quite evident that the aneurism had given way, not alone from the sensation of the patient, but also from the altered condition of the popliteal region. Pulsation was distinctly traceable in it, but there was a tense swelling, which not alone occupied the ham, but also extended along the inner and lower fourth of the thigh and the upper part of the calf of the leg. Deligation of the femoral artery was decided upon; and it may be here remarked that the several stages of the operation were materially complicated by the local effects of the compression, which, influenced not alone the depth of the artery from the surface but also the condition of the sheath, the areolar tissue between the artery and the vein being deeply ecchymosed. Two arteries were tied, the first in the line of the femoral, with its anatomical relations, obvious to all present, only producing a temporary suspension of the pulsation in the aneurism, when a second was discovered, external to, and on a place posterior to it, around which a ligature was passed, completely and permanently obliterating the pulsation in the aneurism.

The progress of the case for the succeeding fortnight was most

satisfactory; and at the time of the separation of the respective ligatures—one on the 14th, the other on the 18th day—every hope was entertained that the absorption of the contents of the diffused aneurism was being effected, when the ordinary signs of suppuration of the sac manifested themselves. An opening was made into it, and a quantity of dark grumous fluid, mixed with a few flakes of coagulated blood, escaped; and again, from day to day, sanguine hopes were being entertained of a favourable result, when, suddenly, it was accidentally perceived by the man that some fluid was passing along the limb, and on examination all about was found saturated with bright arterial blood. The hemorrhage was temporarily controlled; but quickly recurred, and escaped by large masses of semi-coagulated blood. There was now no alternative, amputation of the thigh alone affording any prospect of saving life.

Dr. Fleming now directed attention to the prominent pathological features disclosed by the dissection of the amputated limb. The amount of œdema was trifling. In the calf of the leg between the gastrocnemius and soleus muscles an enormous sac, filled with dark coagulated blood, was visible. It extended throughout the popliteal space, and along the inner hamstring muscles, completely obscuring the artery and vein, and the aneurismal tumour, now reduced to the size of a walnut. There was a rent at its inferior portion, and a total deficiency of arterial structure for at least an inch and a-half between it and the posterior tibial artery. The popliteal, about two inches above the sac, presented two small aneurismal dilatations. The remainder of the sac contained laminated coagula.—*November 24, 1866.*

Gangrene of the Lung.—DR. LEET exhibited a recent specimen of gangrenous abscess of the lung taken from the body of Corporal King, 9th Lancers, who was admitted into the General Military Hospital, Dublin, November 10, 1866, from his Regimental Hospital, Arbourhill, with the statement that he was a well conducted soldier, of temperate habits, and had been in hospital six days with influenza; but severe rigors having set in on the 8th instant, he was sent up to the garrison fever wards.

On admission he appeared like a man in delirium tremens; face flushed; eyes congested; tongue coated with moist white fur, and trembling when protruded; skin hot and sweating; pulse 110, soft and easily compressible; bowels confined; manner excited; constantly asserting that he sees faces looking at him through the windows. Careful examination of the chest fails to detect anything abnormal in the physical signs, beyond a few sonorous râles in both bases posteriorly. He has had no sleep for two nights.

November 11.—Tongue and skin same as yesterday; had one thin

motion; heart's impulse feeble; pulse 112, irregular; bronchial râles increasing, but no dulness on percussion; urine scanty, deep red colour, no deposit; prostration considerable.

November 14.—Several large maculæ, fading under pressure, are observed coming out to-day on the trunk; all the symptoms of well marked typhus fever are now present; urine daily measured and examined, contains excess of urea.

November 17.—During these three days the fever has been increasing in severity; prostration greater; has constant muttering delirium; tongue coated with brown fur; unable to sit up from feeling of syncope; secondary pulmonary congestion not increased; chest has been examined every day.

November 19.—Marked improvement in the general strength, and the eruption is not so livid, though it is now permanent under pressure, and a few petechiæ have appeared on the chest posteriorly. He complains of severe catching pain, during inspiration, below the right nipple. There is no dulness on percussion, but a loud coarse friction sound at that point, and posteriorly over lower half of right lung; well marked comparative dulness; bronchial respiration and bronchophony; the sonorous râles, so distinct yesterday morning, have disappeared; respiration audible throughout left lung, accompanied with muco-crepitating râles; respiration 36; pulse 112, feeble as usual; has occasional distressing paroxysms of coughing; expectoration very scanty; frothy mucus.

November 21.—Was seized with severe rigors, followed by sweating; at 11 a.m., he says the chest pain is now slight, but he has difficulty of breathing. A physical examination of the chest, gave the following:—*Right side*—posteriorly, slight comparative dulness; the bronchial respiration and bronchophony have given place to coarse vesicular breathing and intense moist râles anteriorly; at seat of pain all friction sounds gone; respiration almost inaudible from clavicle to mamma, and percussion note remarkably clear; left lung congested as usual; sputa scanty, purulent, not fetid; extensive suppuration in the cellular tissue around the left parotid gland; the abscess was now lanced, and discharged healthy pus.

November 25.—During the last four days the poison has been getting the mastery, and nature giving ground, ague-like paroxysms are continuing; prostration is very great. It is to be regretted no examination could be made posteriorly, as no doubt the physical signs would then clear up the diagnosis, but he implored not to be moved on to either side, the coughing would become so distressing and the chest pain so intense. The front of the chest has been daily examined, the percussion note, from clavicle to nipple, has been increasing in clearness, and is now decidedly tympanitic; respiration is quite inaudible, and no amphoric phenomena present; delirium, brown tongue, dusky face, livid eruption, and dark

purulent sputa, with occasional diarrhea, show how the typhoid state is deepening.

November 28.—Extremely prostrate; dyspnea greatly increased; cannot expectorate; pulse 120, thready; but intellect clear; all delirium gone; chest full of muco-crepitus; suddenly complained of acute pain in the chest, and almost immediately died.

Post mortem examination twelve hours after death—body greatly emaciated; mulberry eruption and petechiæ all over trunk. On removing the skull-cap four ounces clear fluid escaped, and on removing the brain five ounces more ran out from its base. Brain and membranes apparently healthy. On opening the thorax, gas escaped from right pleural cavity; the lung was greatly contracted, lying up against the vertebral column, and half concealed by two pints of purulent fluid. The right pleura costalis had not lost its smooth polished appearance; it was moist and thin, and could be easily detached from the costal cartilages. No pleuritic adhesions on either side of thorax. On carefully turning up the lung a large gangrenous abscess, pouring out its contents, came into view; it had no communication with either right or left bronchus. The left lung was congested; liver enlarged; spleen enlarged and softened; kidneys congested; several superficial ulcers along the lower end of ilium. No other abnormal appearance was observed.—*December 1, 1866.*

Fibrinous Tumours.—MR. COLLIS exhibited a small tumour which he had assisted Dr. Kidd to remove from the arm of a lady about ten days ago. The tumour was pedunculated, and hung down considerably. The history of the case was simply this:—The tumour had appeared in the cicatrix of an old wound of three or four years standing, whence a tumour, said to have been fatty, had been removed. It had grown to a considerable size, particularly within the last six months, and during this increased rapidity of growth it caused much pain. The tumour presented characteristics sufficiently remarkable to warrant him in bringing it before the Pathological Society. Between the group of fibrous tumours and cancer there lay a class of tumours to which a great variety of names had been given according to the fancy of writers. Many of these names might be discarded, while others might very fairly be retained. Among those which might be retained were the fibro-plastic tumours, which, under the name of myeloid, were known as connected with the periosteum, and also the keloid growth, which was known as the cicatricial tumour. He was disposed to class this tumour, at first, under the keloid variety; but on closer inspection he found it was not so much on the skin as the keloid tumour, but was beneath the skin, though it presented to the eye the rosy colour and to the touch the firm elastic feel of the keloid tumour. He thought the term fibrinous well applied to this form of tumour. Fibro-plastic might be applied to those connected with the

periosteum, and it might be better not to disturb it from that connexion. He thought it better to give them the name of fibrinous tumours, as this implies their origin from the lymph cells, and is at the same time a term sufficiently comprehensive to include many forms of morbid growth which differ slightly from each other, and to which various names had been given, according with these slight differences.

They found the section of such a tumour to be dry. There was no cream or juice exuding from it as in the scirrhus or cancerous tumour. The substance of it was firm and elastic to the feel, white and shining in its recent condition, with sometimes a rosy tint through it. The convex surface of the section showed the great elasticity of the tumour. They found it composed of fibro-elastic tissue, with a large proportion of fibrine not developed into fibre. The peculiar form of cell, mixed with the fibro-elastic tissue, was in this case of the oat shape, a form shown by Paget to be connected with a tendency to recur; tumours composed almost exclusively of it were known therefore under the name of recurrent tumours. Mr. Collis thought if they were to group all these tumours under one name, and then to indicate the specialties of each tumour, it would be better than having a multiplicity of names which only led to confusion. This special form of tumour was rather rare. They found it generally occurring in connexion with injuries or hurts of the skin, frequently with cicatrices—in this, resembling the keloid, which was essentially in the skin.

He might mention that in order to obviate, if possible, the recurrence of this tumour, they smeared the surface of the wound with a solution of thirty grains to the ounce of chloride of zinc, a mode of treatment of these wounds to which attention had been lately called by Mr. Campbell de Morgan.—*December 1, 1866*

Fracture of the Pelvis.—DR. QUINLAN brought under the notice of the society a case of extensive fracture of the pelvis, accompanied by diastasis of the symphysis pubis, rendered more remarkable by the fact that, notwithstanding the extent of the osseous lesions, there was no injury of any of the pelvic viscera. He said that the specimen was taken from the body of a female, aged thirty-five, who was, upon the 18th of November, knocked down and run over by a horse, yoked to a heavy country cart, which was, however, empty at the time of the accident. Her symptoms were at first those of collapse; and when reaction had set in it was found that there was a fracture of the ala of the left ilium and a counter-fracture of the right os innominatum; the fracture of the left side being produced by the direct violence of the wheel passing over that side of the body, and that of the right by the pelvis being “stove in” by the agency of the head of the right femur, which, by means of the right great trochanter

resting upon the ground, acted as a wedge. Swelling and œdema of the right thigh and iliac region followed, and ended in a deposition of matter underneath the fascia of the right iliac fossa and down along the sheath of the vessels of the right thigh. There was also a great deal of localized peritonitis in the right iliac region. The patient finally sank, from pelvic cellulitis, upon the eighteenth day.

A *post mortem* examination was made twenty-four hours after death; and upon opening the abdomen there were found, in the right iliac region, a fluctuating swelling, to which the cecum was attached by lymph, the result of the circumscribed peritonitis above alluded to. The cecum was dark-coloured and inflamed, but there was no communication between it and the swelling. Laying open this latter there was found a great deposit of purulent matter, which was traced along the psoas muscle as high as the fourth lumbar vertebra, downwards, half way down the thigh, along the sheath of the vessels, and inwards to a fracture of the os pubis, which will presently be described. This cavity contained, in addition to pus, some aplastic lymph and ecchymosed blood. Along the right ilio-pectineal line there was extensive ecchymosis under the peritoneum, as if the wheel, after leaving the ala of the left ilium, had pressed down upon this part. The bladder, vagina, uterus, rectum, and intestines were carefully investigated, and were found uninjured. Upon examining the osseous structures of the pelvis there was found, in the left os innominatum, a fracture extending from beneath the anterior inferior spinous process to a point about an inch behind the anterior superior spinous process, and breaking the piece off. There was another fracture commencing about an inch in front of the posterior superior spinous process, and running into the left synchondrosis. In the right os innominatum, where the counter-fracture had occurred, there was a longitudinal fracture, commencing at the upper and outer part of the obturator foramen, and extending backwards through the acetabulum and the ala of the ilium to the crest, at a point about two inches in front of the posterior superior spinous process. From the centre of this fracture there was another, extending through the ala of the ilium and the anterior superior spinous process. Turning to the acetabulum, the great longitudinal fracture, already described, was found passing through this cavity in a line slightly above its notch. From this there branched off a fracture of the shell of the acetabulum, running across the whole cavity. The cartilage of the head of the femur opposite these two fractures was eroded; and around the erosis the cartilage was very vascular. In the right pubic ramus there were two fractures, one of the thin portion of the descending ramus a quarter of an inch above the point of fetal union, and the other of the ascending ramus of the ischium where it joins the tuberosity. Lastly, there was a dislocation of the symphysis pubis, the right pubes being dislocated forwards and

slightly upwards, the inter-articular cartilage remaining attached to the left pubes, and having still connected with it a fragment of the right pubic bone. The sacrum and coccyx were uninjured.—*December 8, 1866.*

Cholera.—DR. HAYDEN exhibited some morbid specimens taken from patients who had died of cholera in the Mater Misericordiæ Hospital in the course of the present epidemic. It appeared to him that this subject, which was to a certain extent new to the society, should be illustrated by all the morbid specimens obtainable, and therefore he had thought these worthy of being brought under their notice. The first was taken from the body of a man, aged twenty-six. He was taken ill with all the symptoms of cholera on the morning of the 20th of September. He was admitted two or three hours subsequently, viz., at ten o'clock, in a state of collapse, and on the following morning at two o'clock he died—that is, in nineteen hours after the commencement of the illness. The symptoms which he presented were those of perfect collapse, with all the appearance of typical cholera. A *post mortem* examination was made five hours after death. The body was then much less livid than it was before death; there was no rigor mortis. The sinuses of the dura mater and the cerebral veins were engorged with dark blood. In the cerebral ventricles was found a large quantity of clear serum—indeed, they were distended with it. On making a horizontal section of the hemispheres of the brain a remarkable appearance presented itself; the puncta cruenta were large and numerous, and from each dark fluid blood welled up immediately, and spread over the surrounding surface, so as to form a circular patch of ecchymosis, as large as a three-penny piece, of which the divided vessel formed the centre.

The vessels of the pia mater were somewhat engorged. The lungs were congested; more markedly so on the right side than on the left. The right lung presented a slate-coloured appearance posteriorly and inferiorly, where it was filled with dark blood mixed with serum, but still resonant on percussion and slightly crepitant. The heart was engorged—its right cavities filled with dark blood. In the right ventricle this was enclosed in an envelope of firm yellow fibrine one-eighth of an inch in thickness.

The left auricle was contracted and empty. The left ventricle quite empty, its cavity diminished in size, and its walls greatly thickened. In the abdomen the most remarkable appearances presented themselves. The liver, spleen, and kidneys were perfectly normal. The external surface of the hollow viscera was smooth, white, and glistening, as in a state of health.

The mesenteric glands were considerably enlarged, some of them as large as peas, others the size of duck-shot, but all more or less enlarged. The stomach contained a quantity of gruel-like fluid. The œsophagus,

at its entrance into the stomach, presented a remarkable appearance, the mucous membrane projecting into the stomach, forming a radiated disc with a festooned border, and presenting the appearance of a piece of wet parchment. It was pale in colour, and sodden-looking. Immediately round the cardiac orifice of the stomach the mucous membrane was remarkably mammilated, the crypts distended with an opaque white fluid. The small and large intestines were nearly filled with a fluid very like thin gruel in colour and consistence, and containing small light-coloured flakes floating in it. In the duodenum Brunner's glands were found enlarged and of a light colour. In the jejunum and ileum the glands, both solitary and agminate, were enlarged, and filled to distention with an opaque fluid of the colour of unhealthy pus, and presented the appearance like that of the early stage of small-pox. The surface was remarkably vascular in the jejunum and upper portion of the ileum. The last three inches of the ileum were free from vascularity, and one of Peyer's patches was here found to have discharged its contents, all its glandules being ruptured and collapsed, and presenting a distinct aperture on the surface by which the contents had escaped; the entire surface of this patch, which measured about three inches in length, was depressed below, and contracted markedly with, the surrounding surface on which the solitary glands were prominent and distended. The fluid contents of one of these glandules were examined microscopically, and found to consist of large granule-cells like mucus corpuscles, and amorphous granular matter. The enlarged glands extended quite to the free edge of the ileo-cecal valve.

The next specimen which he exhibited was taken from the body of a boy whose age was six years. He was taken ill at seven o'clock on the morning of the 23rd September, admitted into hospital at ten, and died at three o'clock on the following morning, exactly nineteen hours from the date of first illness, as in the former case. His case was likewise a typical one. He was perfectly livid, had no pulse, vomited, and suffered from violent spasms. A *post mortem* examination was made of this boy's body six hours after death, and as in the former case the lividity was less than before death, and there was no rigor mortis. The cerebral veins and sinuses of the cranium were remarkably distended with dark fluid blood. In the ventricles of the brain he did not find so much serum, but still a few drachms of it. On making a horizontal section of the hemispheres the puncta vasculosa immediately became the centres of large patches of ecchymosis. The choroid plexus was of a slate colour, and deeply congested. The pia mater investing the surface of the brain throughout, but more especially at the base, was congested. The upper portion of the spinal cord was in a similar condition, but its white substance and grey centre were free from vascularity.

The lungs were engorged, but not to the same extent as in the first case; the anterior portions were light in colour, and crepitant.

The heart presented an appearance similar to that observed in the first case. The right cavities engorged with dark treacle-like blood, and as in the first case, in the right ventricle it was enveloped in a capsule of fibrine which extended into the pulmonary artery. The *venæ cavæ* were likewise engorged with dark blood; left cavities empty, and the left ventricle diminished in capacity and thickened in its walls.

In the abdomen the appearances were similar to those in the first case. The stomach presented a similar condition. The mucous lining of the œsophagus projected into the stomach, and presented the appearance of a wetted bladder or piece of parchment, and around the cardiac orifice the mucous membrane of the stomach exhibited the mammilated condition mentioned in the first case. In the duodenum Brunner's glands were enlarged, and in the jejunum and ileum the glands, both solitary and agminate, were distended and prominent, as previously detailed, and one of Peyer's patches near the cecum had discharged its contents, all its glandules being ruptured and collapsed, while all of those in its neighbourhood were full to distention with a light coloured fluid like gum-water. In the large intestines some vascularity was apparent.

The liver, spleen, and kidneys were in a normal state. He took one of the semilunar ganglia with the great splanchnic nerve connected with it, in order to test a theory which had been advanced in connexion with this disease. According to some paresis of the vaso-motor nerve system would explain most of the symptoms in cholera, and it had been suggested that perhaps congestion, or other abnormal condition of the semilunar ganglia might be found. Nothing of the kind, however, was to be seen in this instance; it will be observed that the ganglia and appended nerve are, to all appearance, in a perfectly healthy condition. (Morbid specimens exhibited).

The third case was that of a boy, aged four, who also died on the 24th September. In most particulars the details resembled those given of the preceding cases. The condition of the brain was similar, as was also the state of the intestines, which were quite full of a light grey viscid fluid, of the colour and consistence of boiled starch; examined under the microscope this was found to consist of columnal epithelium, and its debris with crystals of triple phosphate. The present case presented the best example of the enlargement, and likewise of one of Peyer's patches of the solitary glands in the stage of elimination. The large intestine was injected, as in the first and second case. He had also obtained from the body of a woman who was admitted in a state of collapse, and with complete aphonia, and who died two hours after admission, the larynx and tongue, with a view to determine whether the loss of voice was explained by some physical change in the organ of

voice. He had previously formed the opinion that the loss of voice was due to debility, or inability on the part of the patient to propel the expired air with the requisite force, but on examination he found that the true vocal chords were thickened, their sharp edges rounded off, and the ventricle of the larynx diminished in capacity, by thickening of the mucous membrane; there was, however, no abnormal vascularity of the larynx.

The first point on which he wished to make an observation was the condition of the glandules of the intestines. There was nothing new in the appearance presented by these. Dr. Parkes noticed it in his work on cholera, but he made a statement which appeared to him (Dr. Hayden) not to be justified, viz., that the enlargement of the glands was the result of the diarrhea. He could not agree in this opinion. He was not aware that enlargement of the glands had been the consequence, although frequently it has been the cause of diarrhea in other diseases, and when they found that in the neighbourhood of these glands there was no appearance of vascularity they could not admit Dr. Parkes' view, which would involve the existence of a considerable amount of irritation and vascularity. The mesenteric glands were enlarged in all these cases. They were secreting glands, as were also Brunner's glands, and yet these were quite as much enlarged as the excreting glands, properly so called.

With regard to the alleged lesion of the vaso-motor nerve system, whilst he would not be understood to deny the possible existence of some inappreciable alteration or functional derangement of that system, he did not think it was good philosophy to attribute to a lesion not proven to exist, a set of symptoms that may be explained by organic changes constantly found. If, in addition they assumed, as he thought they were warranted in doing by all the evidence before them, the existence in the blood of a specific morbid poison, they would have in that, taken in connexion with the lesions he had exhibited, a satisfactory explanation, as it seemed to him, of most, if not all, of the symptoms of Asiatic cholera.—December 8, 1866.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

DR. STOKES, President.

Farther Observations on Hydrocephalus, and its Treatment, particularly by the Use of Issues. By HENRY KENNEDY, A.B., M.B.; one of the Physicians of the Cork-street Hospital, and attached to Sir P. Dun's.

Amongst the diseases of childhood, hydrocephalus occupies a prominent place; and I presume I address few who have not met it, and who will

not acknowledge its great fatality. Some twenty years ago it seemed to me the most fatal disease seen in early life, and must probably be still so considered. Yet, as years have rolled on, I have gradually come to modify my opinions materially; as I now know that a fair share of cases may be saved, even when the disease has passed into the second stage; that is, when dilatation of the pupils has taken place, and there is every reason to suppose that effusion is present. It is quite true that there are a number of cases of recovery on record, even from this stage of the disease. But the most of them all are isolated cases, and many of them published as what might be called curiosities, being recoveries under very hopeless circumstances. Now, I wish to state here that, with our present knowledge, we may look with confidence to a certain per centage of recoveries, and what is of still more moment, that these recoveries are directly due to treatment, of which I hope to give sufficient proof before these remarks are concluded.

When I last spoke on this subject before the Obstetrical Society, I entered, at some length, into several points connected with the disease, such as the prognosis, diagnosis, and above all, the natural history of the affection, on a knowledge of which latter so much of the character of the physician depends. Into these I shall not enter again, and more particularly, as they have already appeared in print. There is one of them, however, to which I must again advert, I mean in reference to the two forms of the disease which are to be seen in practice. In the one—and this, probably, the more common form—the disease comes on in the progress of gastric or infantile remittent fever; or supervenes on an attack of some one of the exanthemata or hooping cough; or as a disease *sui generis*. In the second, the disease supervenes on regular typhus; by which I mean a fever attended with spots. Now, when hydrocephalus comes on in the course of the former, it is, in my experience, a much more fatal affection than in the latter. Some gentlemen I know have thought that the disease arising in connexion with typhus fever was not the genuine hydrocephalus. For myself I must say I cannot distinguish between them; but, as the point is one of considerable moment, it may be well to bring before the meeting a sketch of a few cases, and in this way the members can judge for themselves.

CASE I.—In July, 1855, a boy of ten years of age was admitted into Sir P. Dun's Hospital. He laboured under typhus fever, and had a copious rash over his body. He was sent in by Dr. Mecedry. The patient was quite stupid, and very hard to rouse in any way. There was a marked frown on his forehead, and any complaint made was of his head. Both pupils were dilated; the pulse was rapid, the discharges from the bowels blackish, and every now and then he had the cry which so often marks affection of the brain. This case got well, but his recovery was

of the very slowest, and the dilatation of the pupils was the last sign to give way.

CASE II.—A boy of ten years of age was admitted to hospital labouring under typhus fever, and well spotted. He had also oozing of blood from lips and gums, and was said to be eleven days ill. Two days later, that is, when about thirteen days ill, signs of the brain getting engaged appeared. Severe pain in the head came on, attended by marked screaming, and the pulse, which had been rapid, fell suddenly to 60. The pupils, too, became dilated, though not in a marked degree. It is enough to state that this case also recovered; but that recovery was, as in the last case, very slow, and the pupils remained still dilated, at a time when the patient was otherwise well.

CASE III.—Fitzgerald, a child of seven years of age, was admitted, labouring under fever, of as severe a character as is ever seen in childhood. The tongue was dry as a board, and the lips covered with sordes. There was also tympany; but there were no spots. I could not learn how long he was ill. In a day or two later symptoms of brain affection made their appearance. The pupils became dilated, one more than the other; sighing also was very marked, and grinding of the teeth, and of such an intensity as made it most painful to hear. Then squinting came on, and finally convulsions, and death.

CASE IV.—Cunningham, a girl of thirteen, admitted labouring under heavy petechial fever. Whilst in this state she got a very severe attack of diarrhea, the discharge being of a light yellow colour, and attended with tympany, and marked pain on pressing the ilio-cecal region. Her lips were covered with sordes, the eyes deeply injected, and her nights very bad. In fact, the case was a good example of typhus and enteric fever conjoined. As the abdominal symptoms subsided the patient began to complain of her head. She got pain in it which she referred to the forehead, and this was attended by a very marked frown, with constant and deep sighing. The next day I found her with her head drawn back, and complaining still more of pain in it; and she was reported to have screamed out frequently during the night. The pulse was now become unequal, the pupils had a tendency to dilate, and the eyelids drooped. The treatment—of which more again—succeeded in removing all these grave symptoms, and the patient made a good recovery.

CASE V.—Burn, a boy of ten years of age, admitted to hospital labouring under fever, of what would be called the gastric type. His tongue was very red at the tip and edges; elsewhere thickly coated with a white fur. The eyes were deeply injected, the cheeks of a very

high colour, and he made great complaint of his head. When about ten days ill, a rash, of an unusually distinct character, appeared on his arms and legs, and, subsequently, on the body. This course of the rash, I may observe in passing, is by no means uncommon, and more particularly in childhood. The spots in this instance were large, well-defined, and of the bright colour so constantly seen in the young. Though the fever declined, and the spots disappeared, the head became worse and worse, and there was every prospect of the case proving fatal; as I had seen in too many instances before. Treatment, however, did ultimately prove successful, and the boy made a good recovery.

CASE VI.—A boy, of eleven years of age—one of four brothers, all in hospital at same time—presented very heavy spotted fever. As it declined, his brain became seriously engaged; so much so that his life was in imminent danger for many days. By treatment which would at present be thought very active, the patient gradually emerged from this state, and he made a good, though a remarkably slow recovery.

Did time permit, I could give a number of other cases where an affection of the brain—as I believe, of the nature of hydrocephalus—exhibited itself; either when typhus, as shown by the presence of spots, was present, or just as the symptoms were on the decline; or the patient might even be pronounced convalescent. And here I may remark that hydrocephalus does not exhibit that uniformity of symptoms which books would lead one to expect; or at least exceptions are so frequent that we should be prepared for them. Thus I have seen cases, and they were proved by dissection to have been the disease, where there was no vomiting at all, or only at the very commencement of the illness; and, again, I have seen cases where the pulse did not go through the stages which writers have laid down as diagnostic of the affection; and so of other symptoms, as, for instance, the pupils, which may be seen contracted from first to last. The cases given this evening bear out these remarks, and show, as I believe, some of the varieties the disease exhibits; whilst the rate of mortality, which it must have been observed was very low, proves what I stated at first, that the affection of the brain, seen either in conjunction with typhus, or just as it is declining, is not so fatal as when hydrocephalus occurs under other circumstances. It is not my intention this evening, nor would it be suited to the meeting I have now the honour of addressing, to enter into any farther points connected with either the diagnosis or prognosis of the disease. But to some remarks on the treatment I would ask attention; and, indeed, it was for this special purpose I now appear here.

It is more than a century since the periodicals of the day began to contain scattered cases of hydrocephalus in which recovery occurred under very hopeless circumstances; the change being due to the

administration of mercury. From that period mercury became the established medicine for the disease, and so continued till about thirty years since, when the discovery of iodine, or rather its preparations, led to the use of the hydriodate of potash, in certain stages of the disease. Still I believe that, with many, mercury continues to hold the first place. For myself I may say, I had seen it very largely used; but with the most indifferent success. A case now and then did recover, and, apparently, from the direct effects of the drug. On the other hand, I have notes, at the time of which I speak, of upwards of twenty cases, in all of which mercury was freely given, and yet not a single recovery occurred. With such an experience as this, it will not be wondered at that some change appeared to be needed; and so when I came to have the treatment of the disease in my own hands mercury was not used as I had seen it given; and at the present time it is only as a purgative I order it, and that within the first three or four days of the attack. In its place I substituted what would be called a diuretic plan of treatment; the medicines consisting chiefly of the preparations of potash, to which, as recommended by the late Dr. Cheyne, the tincture of digitalis was commonly added. By this plan a certain measure of success was attained, and this was still further added to when I came to use the hydriodate of potash. The use of this medicine in hydrocephalus I look upon as a very decided advance in the treatment of the disease. But I do not for a moment claim any originality about it. It was given by others before I had ordered it. All I can do is to add my mite of experience in its favour. When I last spoke on this subject, several cases in which this drug was largely used, and in which recovery, under very hopeless circumstances, took place, were detailed; and before I close these remarks others will be given.

In speaking thus of a particular medicine it must not be supposed that the remedies in more common use are to be neglected. Thus there are very few cases where, within the first week of the attack, leeches are not of essential benefit; and I mention these more particularly because it has become so much the fashion of late to decry anything of what is known as the antiphlogistic treatment. In the present instance experience must overthrow theory, no matter how ingeniously supported, or by whom. And equally so of other remedies of the antiphlogistic class. But granting that all the means now alluded to have been put in full force, are they capable of coping with the disease under consideration? I believe not. All they have effected in my own hands has been to render it somewhat less mortal than previously. More were saved; and some under very hopeless circumstances. Still the fatal cases far, in a way, exceeded those that recovered.

It was while matters were in this state, that is, all the remedies in more common use were administered, together with the hydriodate of

potash, I observed something else was required. I could not help being struck with the fact that some of the cases recovered, as far as the brain symptoms went, but died in the long run either from pure wasting, or from disease set up in the chest or abdomen. With such experience, the idea of giving wine at once presented itself; and this I believe to be a very important step in the treatment. All I address must be aware that hydrocephalus is essentially a constitutional affection, and in the great majority of cases closely allied to the strumous diathesis. Hence the greater need of some such treatment as this. In the various works on the disease, through which I have looked, I do not find any mention of wine; except in West's able work, and then in the most casual manner only. I look upon wine, however, now as very essential; and in all the cases I have hitherto given, which were successful, it formed a very important part of the treatment. It was given as early as the symptoms in each particular case allowed; and largely, considering the age of the patients. Perhaps one case of this kind, and treated as I have detailed, will not be out of place. It has, however, been given before.

CASE VII.—A girl of twelve years of age was admitted into hospital, labouring under a very severe attack of typhus fever, being densely spotted. She was very restless, the brain much engaged, and she rambled constantly, and kept the patients in the same ward disturbed by her cries. This state continued much longer than usual, but finally subsided, and the girl recovered so far as to ask leave to get up. At this period she began to complain of her head, referred to the forehead; with this fever lighted up again, the skin being hot, the tongue furred, and the pulse quick. From this on every symptom of water on the brain developed itself. The pulse passed through all the more regular stages of the disease, even to the third; the pupils became dilated; the girl quite stupid, and apparently stone deaf; the arms were rigid, whilst the peculiar cry of the disease was constant. From this state, which lasted about nine days, the patient gradually emerged, and very interesting it was to observe the daily progress. The dilatation of the pupils was the last symptom to yield, and even when she left hospital they gave the idea of being still very sluggish. The entire period occupied in the second attack was, in this instance, just five weeks. Besides the ordinary treatment, which consisted mainly in repeated leeching and blistering, and the steady use of the hydriodate of potash, this patient got wine from an early period of the attack; eight ounces for some days, and then for a longer period six each day. The case was seen all through by Dr. Grimshaw. Successful, however, as this particular case was, I must add that the same treatment failed me in other cases, where I had every reason to hope it might prove of benefit. I found that in such cases I could better the disease, and the patient expressed as much, but I did not cure it. The improvement,

in fact, was but temporary, and the disease again resumed its sway to a fatal termination. In all of these cases the disease seemed to me prolonged beyond the usual period; though this is not an easy matter to determine; and in some of them it certainly spread from the brain, which was the first part attacked, to the spinal marrow or its coverings; and so became a kind of cerebro-spinal arachnitis. This was shown by stiffness in the neck and arms, and occasionally by opisthotonos; neither of which were present when the attack began. It may be observed, in passing, that this course of the disease is very important to recognize, and one for which we should be prepared. It shows we have a very obstinate affection to contend with. At any rate, some of the cases terminate in this way, while in others it was a recurrence of the disease in the brain, after it had apparently yielded to treatment, which proved fatal. Under these circumstances, it appeared to me that an issue might prove of service. All the ordinary means of counter-irritation, including tartar emetic ointment, had failed again and again, and so some decided measure was needed, if any treatment of this kind was at all to avail. This is not the place to enter into an inquiry of the effects of issues. I believe, at the present day, they are too much overlooked, and that they are a most powerful means of combatting disease admits of no doubt, and more particularly when made of large size. With these feelings I determined, in the next obstinate case I should meet, to insert an issue in the head, and see what the result would be. The following cases are examples where this line of treatment was adopted.

CASE VIII.—Gaskin, a girl of ten years of age, admitted into hospital in May, 1866. I was in the ward at the moment she came in, and thought, as she walked towards me, that she was deformed; for her head was drawn forcibly backwards, and severe pain was caused by the slightest attempt to move it. She complained of pain, too, in her head, and said she had been a week ill. There was great distress in her countenance, and a marked frown on her forehead, and the pupils of both eyes were much contracted. With these symptoms there was very high fever and rapid pulse. The patient, I should add, was thin, had a fine skin, and high complexion. Her head was directed to be shaved, six leeches to be applied to the nape of the neck, a turpentine enema to be used, and doses of gray powder, at fixed intervals, to be given. This treatment gave some relief; and two days later, that is, on the 16th of May, she got four grain doses of the hydriodate of potash every three hours; also four ounces of wine. This plan, together with leeches and blisters, afforded marked relief, but only of a temporary kind, for the pain in the head returned, and it was to the forehead all her suffering was, from this time forward, referred. At this period the pupils began to dilate, and there was occasional vomiting. All through the pulse kept

high. For so far the pain in the head was comparatively bearable. It became now, however, much more severe. She would scream with agony, holding her head with both hands, and crying out, "My head, my head." In this dilemma I had recourse to an issue, and a large one was put over the parietal bone, and with a result that was truly marvellous. From that moment all pain ceased. The face had, next day, regained its calm expression; the fever had fallen a little, and she had not vomited. It is enough to add, that this patient made a steady though slow recovery.

This was the first instance in which I used a large issue, and the result was most striking. For, that they stood to each other as cause and effect I could not doubt. Whilst putting in the issue I observed that the scalp all round became very red. The illness of this patient occupied a period of eight weeks. The next, though presenting under somewhat different circumstances, was also of much interest.

CASE IX.—Grace, a girl of eleven years of age, admitted in June, 1866. Not known how long she was ill. When first seen she was in a state of deep stupor, with both pupils largely dilated, and one eye squinting. The pulse was unequal and slow, about 60 in the minute. She was quite incapable of putting out her tongue. Her head was large, or at least appeared so. She had vomited, and this recurred again in the course of her illness. After the head was shaved it was leeches, and she got one dose of calomel, to be followed by a turpentine enema. The next day she was somewhat roused, and when some drink was put to her lips it was clear an effort was made to swallow. In this state the child was put on hydriodate of potash, four grain doses each four hours; she was leeches and blistered several times, and was given ʒiv . of wine daily; and after some days she seemed to be getting on well, when, after an interval of two days, during which she was still more decidedly better, the neck became again rigid, for it had been so before; pain of a severe kind was complained of in the forehead, the fever increased, and the girl vomited. In fact, there was a distinct relapse. I thought well of again applying leeches to the nape, and at once following this up by the insertion of a large issue in the head, and I had now the satisfaction of finding that the disease yielded to the treatment. It is, however to be particularly observed, that there was no marked remission of the symptoms till the issue began to discharge; and this was, of course, several days after it was put in. This girl made a good though tedious recovery.

In this case the recovery was also remarkable; the more so as the disease was very well marked, and was far on in the second stage when first seen. It is also well worthy of notice the amendment which took place under the use of wine and hydriodate of potash; of the relapse which then occurred, and the final recovery of the patient under the use of the issue. The patient was in hospital from the 22nd of June till the 11th of August, that is seven weeks.

CASE X.—Deegan, a girl of ten, came in labouring under fever of the gastric type, and marked by a very red tongue; there were no spots. After some days the fever got better in every respect. In this state she began to complain of pain in the forehead; and with this there was a marked increase of fever; the pulse being quickened and unequal; and the tongue furred; the pupils were contracted. The treatment adopted, which I need not again repeat, had only a temporary effect in subduing the pain, which, after a lull again recurred, and with increasing violence; and finally the patient screamed with the agony, holding her head with her hands, and I need scarcely say disturbing the patients in the ward. At this period, when the girl attempted to sit up, her head shook violently, and quite beyond her control. I now had recourse to a large issue, placed over the parietal bone; and here, as in the other cases, the relief was very marked. The patient was quite free of pain for two entire days, when it recurred slightly, and so at intervals for the next week; but never in any degree comparable with what it was before the issue was used. She did, however, at this time complain of severe pain in the spine, about the tenth dorsal vertebræ. This is a symptom of grave omen in these cases, as I have too often seen, as showing a tendency in the disease to spread. It was fortunate, however, that it yielded to some local treatment, and the discharge from the issue being now established she made no further complaint of it. About a week later her friends removed her from the hospital, against my wishes, for she had been getting half a pint of wine daily for a long period, together with other nourishment, and it was not likely her friends could give her such at home. I am glad to say, however, that the girl seems now to have quite recovered. She has left two months, and I have visited her twice, and found a very marked improvement in the interval. She had been using the cod-liver oil. It may be well to add that, in this instance, the issue acted a little too deeply; for when the slough came out a small portion of bone was denuded.

The next case presents features of somewhat a different kind; but it is too closely connected with my present subject to be omitted.

CASE XI.—In August last, when I took charge of some of the wards in Sir P. Dun's hospital, a girl of thirteen years of age came under my care. She had been previously a patient of Dr. Law's, and presented then a well-marked instance of spinal arachnitis. When I first saw her it was plain the disease was not not entirely conquered. There was still some fever, with hot skin; furred tongue, and quickened pulse; the child vomited occasionally; and the appetite had not returned. She was put upon the hydriodate of potash. After some days no amendment had taken place. She vomited at regular intervals of about thirty-six hours, and this in spite of anything I could devise to allay it—and a good many

means were tried. The urine, I should say, was free from albumen. Finally she allowed to having pain in the head, referred to the right side, but I suspect this had gone on for days previously, for I had found her crying on several mornings. Though temporarily relieved by leeches and blisters, the pain became gradually more severe, and more continuous, and was constantly referred to the same side of the head. In this state a large issue was put over the right parietal bone; and here, as in some of the other cases, the result was most striking. The pain and vomiting at once ceased, and never returned, and the child made a very good recovery, and got fat before leaving hospital.

With only one other case will I trouble the meeting at present. It has been, however, a very striking one, and might be said to be more a case of spinal arachnitis than hydrocephalus.

CASE XII.—On the 15th November last a girl, named Sadler, aged thirteen years, was admitted into the hospital. At the first glance she had all the look of a patient in typhus fever. She was covered with spots, and her lips had a quantity of sordes on them; also the tongue. In addition, however, to these symptoms, she lay with her head drawn forcibly backwards, and the nurse stated that when she moved her the whole body was stiff. All her suffering was at this period referred to her head and nape of the neck. The skin was very hot; the pupils contracted to a pin's point; whilst the pulse beat but sixty in the minute, and was quite equal in its beats. The girl had vomited, but there were some doubts as to the duration of the attack; she was certainly eight days ill; most probably longer. It would be loss of time to give the particulars of treatment; except to say that it was the same as in the other cases, and with like results, that is, temporary benefit; but nothing I could devise succeeded in conquering the pain in the head; referred in this instance to the forehead. She had also at times severe pains in the spine, and radiating into both the upper and lower extremities. All through her illness hitherto, that is for a period of twelve days, the pupils kept very much contracted, and the pulse slow. In this condition a large issue was put in the head; this was on the 27th of November. Though a certain amount of benefit arose from this measure, it was not so marked as in some of the other cases. The pain recurred several times in the forehead, and more particularly at night; whilst there was occasional vomiting; and these went on till a discharge began to be established, when they at last ceased, and the girl for the last ten days has made a steady, I might almost say, a rapid amendment. She is still in the hospital, and is getting fat.*

* This girl left hospital on the 21st of January, 1867, having completely recovered. Her illness occupied nine weeks.]

Though five successful cases have been now detailed, the meeting would take up a very erroneous idea if they suppose that all the cases in which issues were used were successful—very much the contrary. It so happened that in May and June of the present year there was what might be almost called an epidemic of affections of the brain of the gravest character. Many of these were sent in at so advanced a stage of the disease that, though issues were used, they proved of no avail, nor any other measure I could devise. From what has been detailed this evening I cannot, however, help thinking that some of these cases would have been saved had they been seen sooner. For I think the cases given by myself prove the very great power possessed by issues; and, whilst I never had occasion to regret their use, I had cases where it was matter of regret they had not been used, or at least at an earlier period. One such occurred in Sir P. Dun's hospital in the end of August. A child of five years old was admitted in the collapse stage of cholera. As it came out of this stage it began to exhibit signs of brain affection; this I at first thought was due to uremic poisoning; but after two days it was clear some other mischief had arisen, for the pupils had dilated, which I had not witnessed where uremia existed. At this time an issue was inserted, but it was too late to save the child. The *post mortem* examination, which was made by Mr. Collins, showed the case was one of hydrocephalus; the morbid appearances being very marked, but confined chiefly to the base of the brain, where the arachnoid was opaque, and had gelatinous effusion under it. In this instance two precious days were lost, owing to the obscurity which hung over the diagnosis. And so of other cases where they were seen too late.

It will have been observed that in the course of these remarks I have spoken of hydrocephalus and arachnitis as if they were one and the same disease, and this was intentionally done. Not that the acute form of arachnitis, whether of the brain or spine, and which prevailed some years since in Dublin, is the same as hydrocephalus, as it is usually met; but that there is a sub-acute form of the same disease which, when it affects the arachnoid of the brain is essentially the disease known as hydrocephalus. In this state there may or may not be effusion of serum; this would usually seem to depend on the duration of the case; but whether there be or not I believe the disease known as hydrocephalus to be essentially a sub-acute inflammation of the arachnoid, occurring very generally in a strumous constitution, and its duration to average from eighteen to twenty-five days. This is the disease about which I have been speaking, and in which I have found issues of such signal use. It is the disease, described by Cheyne, and so many other writers, and which so frequently, but by no means constantly, passed through the three stages they have described.

In conclusion it may be well to state that the issues put in were of

large size, from an inch and a quarter to an inch and a half in length and three quarters of an inch in breadth, and that they were commonly placed over the centre of the parietal bone. They were put in with *potassa fusa cum calce*; but since reading this paper it has struck me that valuable time might be saved if they were put in by incisions. A purulent discharge would, I should say, by this plan be much more rapidly obtained, and where the case was seen late this would be an object of the greatest importance.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.^a
 TWENTY-NINTH ANNUAL SESSION.

DR. SAWYER, President.

ADDRESS OF DR. M'CLINTOCK, PRESIDENT FOR THE PAST SESSION,
 ON RESIGNING THE CHAIR.

GENTLEMEN,—In compliance with the rule of our society, which requires that the outgoing President should inaugurate the incoming session, I most reluctantly appear before you.

Although this by-law may have some advantages, still it has this obvious disadvantage, that it must occasionally expose you to the infliction of a dull or uninteresting discourse; and such, I fear, will be your lot this evening.

My unwillingness to enter upon this task does not prevent me feeling, however, greatly honoured by so large an attendance of members, and by the presence of so many distinguished guests. I am especially gratified at seeing around me not a few of our brethren—the pure physicians and surgeons—for it shows that socially, as well as philosophically, midwifery combines the two great branches of which they are distinguished representatives. Midwifery is, in fact, the offspring of both; for though medicine may justly claim to be the mother of midwifery, yet the active vivifying influence of surgery was required to make it capable of an independent existence and thorough development.

In the position which, through your goodness, I have the honour to occupy this evening, my first duty is to take a cursory review of the past session; and this I do with unmixed satisfaction, for I believe that in every point of view it was a remarkably successful one. The number of meetings exceeded that of any former year, as did also the number of communications, whilst in regard to the variety of subjects brought forward, and the manner in which they were treated, the session was, to say the very least, not inferior to any previous one.

^a These Reports are furnished by Dr. Geo. H. Kidd, Secretary to the Society.

It would be quite superfluous to attempt an analysis of these contributions, as nearly all of them have appeared in the published reports of the society, and it is to be hoped the remainder will, ere long, be in the hands of the printer. In order to confirm some of the statements just made, it is well, however, just to enumerate the different subjects which have come before the notice of the society during its meetings of the last year. They are as follows:—"Unusual Complication of Labour and Pregnancy" (Dr. Ringland); "Labour with an Unruptured Hymen" (Dr. Denham); "Rupture of the Uterus, and Placenta Previa" (Dr. Isdell); "Laceration of the Vagina" (three papers by Dr. Byrne and Dr. McClintock); "Puerperal Fever" (Dr. Telford); "Gynecomazia" (Dr. Foot); "Disease of the Ovaries" (two papers by Dr. Babington and Dr. Telford); "Ovariectomy" (Surgeon Smyly); "Inversion of the Uterus" (Dr. Denham); "Fibrous Tumour of the Uterus" (Dr. Byrne); "Inflammation of Bladder and Vagina" (Dr. Atthill); "Galvanic and other Pessaries" (Dr. Kidd); "An Improved Mode of Preparing Medicated Pessaries" (Dr. Kidd); "The Ophthalmia of New-born Children" (Dr. Wilson); "Double Monstrosity of the Human Fetus" (Dr. McClintock).

We have here a good array of work done; and from this enumeration it will be seen that every branch of obstetric medicine—namely, midwifery proper, gynecology, and paidiontology—was brought under the consideration of the society, and gained some addition in the course of the session.

So far, then, as regards the *matter* of our meetings, now as to the mode in which they were conducted. It is generally supposed that our branch of the healing art more than either of the others embraces topics calculated to provoke conflict of opinion and to excite the feelings or prejudices. But the experience of this society gives a flat contradiction to the above supposition, and I am bound to say—and I do it with sincere pleasure—that in no single instance during the past year had the President to exercise his authority in stopping or controlling the discussion which has followed the reading of the various papers brought forward. To maintain such a tone of courtesy and candour is most essential to the well-being—I might almost say the bare existence—of any debating society. In the last public address he ever delivered, Sir Philip Crampton thus alludes to this very point:—"All the dangers, real or supposed, to which debating societies give rise may, I am firmly persuaded, be avoided, not so much by positive restrictions as by the constant infusion, by precept and example, of a conciliatory and gentlemanly spirit. I can conceive no difference of opinion, in a matter of science, to exist between gentlemen, which may not be expressed, not only without offence, but in such a manner as to excite feelings of mutual respect and good will." If there be an offence against good manners (which have

been well described as the lesser morals), an offence more to be deprecated than another, it is what a great moral poet has alluded to as "the poor renown of being smart":—

"What though wit tickles, tickling is unsafe,
If still 'tis painful, while it makes us laugh;
Who, for the poor renown of being smart,
Would leave a sting within a brother's heart?"

YOUNG.

In so large a society as this, composed of about the most short-lived class in the community, the retrospect of a year will seldom fail to show some gaps in our ranks, some victims to that insatiate enemy whose coming to all of us we are ready enough to acknowledge, but whose proximity to each of us we are so slow to realize. I am happy to say that our losses have been numerically very small, but amongst them was one of the honorary presidents of the society, a man who for a long series of years enjoyed a very large share of public and professional confidence, and was a worthy cotemporary of the many eminent men who, entering upon practice about the same period, flourished in this city within the last fifty years, and whose names will occupy a foremost place among the most distinguished surgeons and physicians of this metropolis. It is, therefore, not only excusable but proper for me, on an occasion like the present, to lay before you some sketch of his history.

I confess I approach this part of my subject with saddened feelings, for in DR. CHARLES JOHNSON I have lost one of my earliest and best medical friends. Whilst, then, I lament his removal, though in the maturity of years and reputation, I yet gladly avail myself of this fitting opportunity of showing respect for his memory, and my grateful recollections of a friendship existing for more than twenty-five years.

Dr. Johnson was a native of the county Wexford, and entered upon professional life by being apprenticed to Dr. Ebenezer Jacob, the first surgeon of the County Wexford Infirmary (opened in 1769), and upon his death, in 1812, Johnson was transferred to Mr. Thomas Hewson, then junior surgeon to the old Meath Hospital "on the Coombe," where the edifice continues to the present day, but, as you are all aware, has exchanged its surgical for obstetrical fame.

Whilst yet a student Dr. J. had a very narrow escape of his life. I relate the incident here, as it tends to enforce the importance of punctuality, which I always maintain is one of the cardinal virtues, inasmuch as order is Heaven's first law, and there could be no order without punctuality. Johnson was sent by his master, Mr. Hewson, to see a patient residing in Mercer-street. Whilst at the patient's bed-side he found, on looking at his watch, that it wanted five minutes of two, at which hour he was to meet the Treasurer of the College of Surgeons—

Dr. Andrew Johnston^a—at the Bank, in order to lodge his fees for the diploma; so, leaving the patient, with the assurance of his being back in a few minutes, he hastened to the Bank, despatched his business, and was returning through William-street, when he was met by the astounding intelligence that the house where he had been visiting, and to which he was on his way back, had actually fallen, and in its ruin had killed the unfortunate patient.

In January of the memorable year 1815 Johnson obtained the license of the Royal College of Surgeons, and in three years afterwards he was elected a member of the college. Owing to some defect of vision in one eye, he was advised to take up the obstetric branch of medicine, as seeing was no part of an accoucheur's duty at that day, the uterine speculum being then unknown. Accordingly, in February of the year 1816, he became an assistant to Dr. Labatt, the Master of the Lying-in Hospital.

Dr. Johnson was fortunate enough to gain, about this time, the friendship of Dr. John Cheyne, then approaching the zenith of his great and deserved reputation, and holding the offices of Professor of the Practice of Medicine at the College of Surgeons, and Physician to the Meath and to the House of Industry Hospitals. Johnson thereby secured not only a patron, whose influence must have advanced him in practice, but a friend whose honest and sound advice was of the utmost value in guiding him through the difficulties and perplexities which every man has to encounter at the outset of professional life. Dr. Cheyne's acquaintance with Johnson began at the Meath Hospital; and it was observing his steady conduct, and more than ordinary abilities, that induced Cheyne to select him to be his assistant at the House of Industry Hospitals, the onerous duties of which appointment Dr. J. zealously discharged for some years. We cannot doubt but that the medical experience he thus acquired, under so accomplished a physician, and in so large a field of observation, was of infinite service to him, and contributed materially to his great success as a practitioner of midwifery, as well as of physic. The acquaintance thus commenced between these two eminent men gradually ripened into a close friendship that continued up to the lamented death of Cheyne, in January, 1836.

^a This gentleman was for many years the Treasurer of the Royal College of Surgeons, as also Professor of *Materia Medica* and Medical Pharmacy (in which chair he was succeeded by the late Mr. Macnamara), and subsequently of Midwifery. In 1794 he got the certificate of the College, qualifying him "to serve as surgeon in any regiment in His Majesty's service." He was appointed surgeon to the 44th Regiment, at Martinique, in 1796. Afterwards he served in the same regiment, in the Egyptian campaign, under Sir Ralph Abercrombie, and received the gold medal of the Sultan. He retired from the army in 1803 (in consequence of ophthalmia contracted in Egypt) and obtained the diploma of the College of Surgeons, licensing him to practise surgery. He died in 1833. For these interesting particulars I am indebted to his son, Dr. George Johnston, of Rutland-square.

Having completed his three years at the Lying-in Hospital, Dr. Johnson took up his residence in South Anne-street, and fairly launched into practice.

Although an interruption to the strict course of the narrative, still I cannot help going over the names of the principal obstetricians of Dublin at that day (forty-seven years ago), the men with whom Dr. Johnson had to compete in the arduous struggle for wealth and fame. The list is pretty extensive, and contains some names that will long be associated with the records of midwifery in this city. First, then, there were two ex-Masters of the Lying-in Hospital, in full practice—namely, Dr. Joseph Clarke and Dr. Thomas Evory, the former in the height of his fame, and still wielding the sceptre of obstetric supremacy with absolute authority, though beginning to contract the circle of a practice that yielded him over £3,000 a-year. Dr. Evory also possessed a large share of public confidence, and, like Clarke, most of his patients were amongst the aristocracy and upper gentry. He was a man in every way calculated to hold a high position in his profession, being not only an experienced accoucheur, but an accomplished physician.

Dr. Labatt, at the time we are speaking of, was near the termination of his mastership of the Lying-in Hospital, with extensive and rapidly-increasing private practice. Dr. John Beatty, though his celebrated paper on the midwifery forceps had not yet appeared, nevertheless was in full business, and reaping a rich harvest as the fruits of his talents and uncommon energy. Dr. Tuke, of Stephen's-green, and Surgeon John Adrien, of Dawson-street, were both in much repute as successful midwifery practitioners, and were largely employed as such by the public of that day.

Besides these prominent men was a number of other able and well-qualified obstetricians, who were all actively engaged in practice—as Dr. Breen, Dr. J. Douglas, Dr. Tom Ferguson, Dr. Pentland (Labatt's successor at the hospital), Dr. Andrew Johnston, the Professor of Midwifery at the College of Surgeons; and Dr. Creighton, his predecessor in the same chair. In addition to these were some others, who had only recently started in the race of competition, but who bid fair, even then, to take a good place—as Dr. Ireland, Dr. M'Keever, Dr. Frizelle, Dr. Shekleton, and Dr. John Peebles—to say nothing of a number of less mark, as Dr. Calonne, Dr. Gilholy, F.C.P.; Dr. Newport, and Dr. Macabe, who nevertheless were well educated and competent practitioners, and each doing a respectable share of midwifery business.

One other accoucheur there was whose name should not be omitted, perhaps, in this enumeration; for, though he was not much to be feared as a rival, yet he was more dreaded by the Doctors than any man in the community, and that was Dr. John Brennan, commonly called Turpentine Brennan, or the wrestling doctor—one of

the strangest compounds that can well be imagined. The history of this man would almost be worth preserving, not merely on account of his trenchant wit and many eccentricities, but as an instance of the power of vilifying in its highest development. There was no degree or species of abuse—satire, ridicule, irony, lampoon, inuendo, taunt, scurrility, or Billingsgate—of which Brennan was not perfect master, and of which, as occasion seemed to require, he did not freely avail himself. He spared neither the physical, moral, or intellectual attributes of those whom he assailed, and the victim of his sallies was most frequently one of his own profession. And yet the *Milesian Magazine*, in which all these specimens of defamatory composition are to be found, and which has scarcely an equal in this style, affords evidence of Brennan's having been a man of strong intellect, keen wit, and classical knowledge.

I am straying too far, however, from my proper subject. We see, then, that at the time alluded to this city could boast of as large a number of well qualified accoucheurs, possessing the confidence of the public, as at any previous or subsequent period. To climb the ladder of fame, and work one's self into first-class practice, with so many able and experienced competitors on every side, most assuredly was no easy matter. And yet, as we know, Johnson did this, and did it, moreover, within a marvellously short interval from the outset of his career.

Of all the men I have enumerated only three survive. Of the rest some have left behind but their name and their example—

Nec vixit male, qui natus moriensque fefellit,—

Others, by their writings, have helped to build up the fame of our national school of midwifery, whilst a few live amongst us in the persons of their sons or grandsons, our worthy friends and *confrères*.

“ While active sons, with eager flame,
Catch virtue at their father's name,
When full of glory, full of age,
The parent quits this busy stage;
What in the sons we most admire
Calls to new life the honoured sire.”

LLOYD.—*Arcadia*.

Instead of confining himself to pure midwifery, Dr. Johnson early resolved to devote close attention to the study of female and infantile complaints—two classes of cases with respect to which our knowledge was then very scanty and inaccurate. Carrying out this wise resolution, we find him, in the year 1822, associated with the late Sir Henry Marsh (then assistant to Dr. John Crampton at Steevens' Hospital) in establishing “The Pitt-street Institution for Diseases of Children,” which, as you are all aware, has existed from that time to the present, and been

productive of much good in various ways. This was, I believe, the first successful attempt in this kingdom to found a special institution for treating the diseases and accidents of early life.^a That it was a proper and needful innovation is shown, I think, by the fact that special provision now exists for relieving such cases in many of our large hospitals. and there is no doubt this mode of collectively studying infantile, or any other class of complaints, has been of great service in advancing medical knowledge and instructing medical students.

In this same year, 1822, Dr. Johnson published, in the third volume of the *Dublin Hospital Reports*, two cases of extirpation of the inverted uterus. The boldness and the novelty of the practice successfully pursued in these cases, for which there was only one precedent here, and three or four in England,^b gained him great reputation for skill in the treatment of uterine complaints. From this time forward a large part of his private, and especially of his consultation practice, was made up of these two classes of cases—namely, diseases of women and diseases of children.

Although the paper just alluded to on *inversio uteri*, and a very admirable one on whooping cough, in *The Cyclopædia of Practical Medicine*, are the only contributions to medical literature that Dr. J. published, still I think we are under some obligation to him for that most excellent and useful manual, *The Dublin Practice of Midwifery*, as the author, Dr. Henry Maunsel, had been an apprentice of Johnson, and, therefore, it is reasonable to suppose, derived his obstetric knowledge from him; at all events the practical teaching of this work was in strict accordance with the views of his master.

Dr. J. was one of those persons who seem to entertain a sort of morbid dread about submitting their opinions or their composition to the ordeal of public criticism. Other instances of this peculiarity in members of our own profession will, no doubt, occur to you; and in Dr. Johnson's case, as well as in theirs, this reticence is just cause for regret, as a man of his vast experience, accurate observation, and matured judgment, could not fail to have added many an instructive page to the didactic literature of medicine. The papers he did publish, as well as his lectures, and indeed everything he wrote, were distinguished for simplicity and clearness of composition. In this respect his style was truly indicated by the legible, smooth, and beautiful execution of his penmanship. Although he was nothing of an extempore speaker, and had done little as an

^a In London there existed, prior to this date, at least two institutions where children's cases were especially attended to: these were, the Royal Infirmary for Children and the St. Marylebone General Dispensary.

^b Dr. Joseph Clarke it was who had previously operated on a case here. Johnson subsequently removed the uterus, for chronic inversion, on four occasions.—*Vide* page 107 of Dr. M'Clintock's *Clinical Memoirs on Diseases of Women*.

author, still, like his namesake, the great lexicographer, he possessed, in an eminent degree, the faculty of expressing himself in the most terse and pithy language, condensing, within the compass of a few words, the keenest sarcasm, the bitterest irony, or the most weighty practical truth.

In 1829 Dr. J. obtained the licence of the King and Queen's College of Physicians, and in 1841 he was elected an honorary fellow. This was a rare distinction, as out of the eighty-two honorary fellows elected since the institution of this order in 1728, not more than five or six were surgeons.

The Professorship of Midwifery in the College of Surgeons having become vacant in the year 1828, Dr. J. applied for it, and was the successful candidate. For nearly ten years previously he had been a member (or, as it is now termed, a "Fellow") of the college, and had served for some time on the Court of Assistants, as well as otherwise taken a part in the administration of the affairs of the college.

Though it may appear somewhat in the light of a digression, still I cannot help taking a retrospective glance at the course pursued by this college in reference to the branch of medicine with which our society specially interests itself. I gladly seize this opportunity of doing so, as I think it redounds to the credit of the Irish College of Surgeons, that from the commencement of its school it made provision for the proper study of midwifery, and that so far back as 1784, the year of its incorporation, it took a just and enlightened view of the importance and dignity of this great department of the healing art. Now, let it be borne in mind that at the period referred to instruction in midwifery was ignored by some medical bodies, and the practice of it regarded as a degradation and positive disability by others.

Among the first professorships created in this college was one of midwifery, according to resolutions adopted in August, 1785. Through want of financial means these resolutions were not carried into effect till 1790, when the school of surgery opened in good earnest, and Mr. John Halahan, the professor of anatomy and physiology, and an ex-assistant of the Lying-in Hospital, began regular courses of lectures upon midwifery in the college school, and continued to do so till 1793, when the college deemed it expedient to appoint a special professor to the chair of midwifery; and for this purpose they selected another ex-assistant of the Lying-in Hospital, Sir Henry Jebb, who was an active member of the college, and subsequently its president. From that time to the present there has been an unbroken succession of obstetric professors and of regular courses of lectures on midwifery in the college school. Sir Henry Jebb was succeeded, in 1795, by Dr. John Creighton, who held the chair till 1818, when Dr. Andrew Johnston, ex-assistant of the Lying-in Hospital, replaced him; and it was on his resignation, in 1828, that Dr. Charles Johnson was appointed, as before stated. He

retained the chair for six years only, as he found that with the immense practice he then had, his health would not be equal to the duties of a professor. Dr. Henry Maunsel, Lecturer on Midwifery in the Park-street School of Medicine, was next appointed, in 1835; and on his retirement, Dr. Beatty, who had succeeded him in Park-street School, was elected, and retained the chair up to 1857, when the vacancy which his resignation caused was filled up by the appointment of Dr. Sawyer.

Dr. Johnson was very successful, both as a medical and obstetrical practitioner. Endowed by nature with an extraordinary amount of sagacity and penetration, he seldom failed to take a clear and comprehensive view of a case. In discriminating between accidental and essential symptoms, and in unravelling the causation of chronic diseases, he often exhibited great acuteness of observation and depth of judgement. He possessed the true *mens medica* in as high degree as any physician I ever was acquainted with. Added to this were the signal advantages arising from study, extensive clinical experience, and habits of careful reflection. He had large confidence in the power of medicinal agents, and as might, therefore, be expected he was most cautious and discriminating in the use of remedies. Hence his prescriptions were admirable, and fully came up to the standard of excellence laid down by that high pharmacological authority, Dr. Paris, who says that the perfection of a medicinal prescription may be defined by three words—*precise* (in its directions), *concise* (in its construction), and *decisive* (in its operation); and such were Dr. Johnson's prescriptions, with the additional recommendation (not always observed by prescribers) of being most legibly written.

He exercised very absolute authority over his patients, and was extremely jealous of any interference with his management; and very impatient—sometimes even to bluntness—when suggestions on the subject of treatment or diagnosis were offered by well-meaning friends about the invalid.

From his first starting in practice Johnson was most particular in the selection of his nursetenders, and would not permit his patients to employ any he did not himself approve of. This was then rather a novel course to adopt, and was often stoutly opposed by the ladies, who considered they ought to have as full liberty in the choice of their monthly nurse as of any domestic servant. But Johnson steadily adhered to the rule he had laid down. By so doing he was able to keep a very wholesome and needful check over this class of medical subordinates; and we accoucheurs of the present day are much indebted to him for possessing an undisputed right of nominating the nurse, without which we should have no real control or authority over her or the patient.

That Dr. J. attained the highest rank in this city as an obstetrician is probably known to most of those whom I have the honour of addressing; and yet all through life he had to contend with a delicate constitution.

He was frequently obliged, from illness, to give up practice for a time, and to recruit his health by rest or travelling. Nevertheless, such was the amount of confidence placed in him by the public, that these interruptions never seemed to entail any permanent loss in his practice, for no sooner did he return home than his patients all came back to him—a constancy on the part of the public that few medical men can boast of.

Another remarkable circumstance in his medical history was the extraordinary rapidity with which he got into large and remunerative business. At the age of thirty, which was about five or six years after leaving the Rotunda, his professional engagements were very extensive, and what is more remarkable, were chiefly among the upper classes of society.

Although he always showed a prudent deference to public opinion with respect to those matters relating to dress, manner, outward appearance, &c., unimportant in themselves, yet regarded by the public as reliable indications of a man's character and capacity; still, his was not by any means an abject, fawning, subserviency. The hair-powder, inexpressibles, and long boots in which the leading obstetricians figured half a century ago he never adopted, and he was the first accoucheur to relieve us from the *chokedom* of white cravats, by demonstrating in his own person that a black silk tie was quite compatible with the highest obstetric attainments. Such was his independence of mind and self-reliance that he thoroughly despised all the indirect expedients for puffing (as it is called), and he held in just abhorrence every means of obtaining reputation or practice but such as accorded with the honour of his profession, and with a high standard of the *amour propre*. In this point of view the career of Dr. Johnson is instructive and well deserving of our approval; for though there may have been a mixture of pride in the resolution to acquire fame without fictitious assistance of any kind, nevertheless it is a commendable species of pride, and we must admire the man for the constancy and the success with which he carried out his determination, and earned character and wealth by toiling up the arduous road of patient industry, well-directed application, and strictly honourable conduct towards his brethren. After all, gentlemen, I believe this is the only secure basis on which to build a permanent reputation in medicine or any other scientific pursuit.

Of Dr. J.'s merits as a midwifery practitioner I had large opportunities of judging, having been under him in the Lying-in Hospital for upwards of four years, and having frequently met him in private practice. He held in reverence the maxim that "meddlesome midwifery is bad," and certainly he was much opposed to the early employment of instruments. Though entertaining a somewhat exaggerated idea of the dangers of the midwifery forceps, still he was otherwise free from any prejudice against

the instrument; and this, it must be admitted, showed no small independence of mind, considering his early training, and the obstetric teacher under whom he was brought up. He was wont to say that "it was one thing to be able, *quoad* the case, to use the forceps, but quite another thing to have justification for its use."

What to consider as justification for its employment in tedious labours is a question which has divided the obstetric body into two great sections. The more modern and larger party hold that the instrument should be used in *anticipation* of the ill effects which delay may give rise to. The other great section—which includes the names of Collins and Robert Lee—consider that we should not resort to the use of the iron hand (as the forceps has been well called) till the failure of the natural powers is unequivocally demonstrated. This latter view was held by Johnson, but in a very qualified manner only, for he sanctioned (as Dr. Hardy's and my Report amply proves) a much more frequent employment of the forceps than would accord with the principles of either of the great authorities I have named.

Though myself an advocate for the employment of this "noble instrument" (to use Chapman's designation of it) as a prophylactic measure, still my experience of a clinical obstetric school would incline me to put up this motto conspicuously in every ward of a lying-in hospital frequented by pupils, "Meddlesome midwifery is bad;" and those who have had a like experience will, I doubt not, see the reason for this, and admit its necessity. Johnson's great forte was in puerperal diseases. Here his skill in diagnosis and his accuracy of prognosis were unsurpassed. His opinion in such cases was truly valuable, and that it carried great weight was fully shown by the large number of puerperal cases he was asked to see in the course of his consultation practice.

In November, 1840, Dr. Johnson was unanimously elected Master of the Lying-in Hospital, in which place and year my acquaintance with him commenced, and continued uninterruptedly to his death, on the 19th June last. From him was derived nearly all my obstetric knowledge: his advice often guided me in times of doubt and difficulty, and his kindly interest in my personal welfare seldom failed to inspire me with fresh courage and hope when cast down or despondent.

An experienced and faithful friend at the outset of life is of inestimable value. Such was Dr. Charles Johnson to me, and therefore he must ever hold an honoured place in my remembrance.

"My thoughts are with the dead; with them
I live in long-past years,
Their virtues love, their faults condemn,
Partake their hopes and fears;
And from their lessons seek and find
Instruction with an humble mind."

SOUTHEY.

Gentlemen, I fear I have occupied you a long time with the retrospective portion of this address. I do not offer any apology, however, as I cannot see how it could well have been otherwise.

Of the present condition and prospects of our society there is no need for my saying much, as you have already heard the Council's report, which, though dry in manner, as such a document should be, yet must gratify us all, inasmuch as the statements it contains furnish the best evidence of the continued prosperity of the society and of the increasing estimation in which it is held by the profession at large.

The *renaissance* of our society, in 1862, not only marks an important epoch in its history, when its constitution underwent a complete remodeling, but it was the commencement of a new era in which the society has displayed a vitality, an energy, and a growth that have exceeded the expectations of its warmest friends.

When, at the opening of the twenty-fifth session, in November, 1861, I ventured to suggest that some alterations had become necessary in the by-laws, and that the whole code should undergo a careful revision, I candidly avow to you I did not at all anticipate what great and beneficial changes were to arise from the movement thus initiated. At the good results attending this reconstruction of the society, perhaps nobody—one alone excepted—could experience more thorough and hearty satisfaction than I have done. You will forestall me when I say that the exception just alluded to can be no other than the distinguished founder, Dr. Every Kennedy. Had the creation of this society—which was the original of all the societies, having the same objects in view—been the only public act of his professional life, and were there no other grounds on which to rest his claims to celebrity, he would still have justly earned a high position among the benefactors of our art.

This society has done immense service during the twenty-eight years of its existence. It has kindled a spirit of honourable emulation amongst us. It has stimulated our industry, has united us more closely in the bond of professional brotherhood, and has been the means of drawing forth many admirable contributions to obstetric science which otherwise would never have seen the light. I feel that our national character as an independent school of midwifery has become identified with it, so that henceforth this society will be regarded as the gauge or index of our earnestness and activity in the cultivation of obstetrics.

The few moments longer that I can hope to keep your attention I would wish to occupy with some general observations upon the subject of *uterine therapeutics*.

The advance in our knowledge of gynecology within the last thirty-five years is, I believe, unexampled in any former age, or in any other branch of the healing art. This extraordinary development has resulted partly from a closer study of pathology, but mainly, I would say, from

improvements in our means of diagnosis, more particularly by the acquisition of the vaginal speculum and of the uterine sound. An additional sense was thus brought to our aid in the investigation of uterine disease, whilst the range of another sense was materially extended.

Hence it is not at all surprising that the diagnosis of every uterine lesion should now be made to rest nearly altogether on objective phenomena. One of the latest writers upon uterine diseases thus expresses himself on this point:—"Of all organs the uterus is now most subservient to the laws of *physical exploration*; and in every case of diseased action, if we cannot map out accurately the peculiar condition of the uterus producing or accompanying it, it is simply because we do not apply our *knowledge of those physical laws* to its investigation."—(Sims, p. 144.)

Now, this habitual reliance on physical signs in the forming of our diagnosis, together with the modern discovery of the frequent existence of misplacements of the uterus, and an exaggerated etiological importance attached to these misplacements, are so many circumstances which, in my humble judgment, have tended to impart too much of a mechanical or physical character to the prevailing views of uterine pathology and to the principles of treating many uterine diseases.

It is matter of notoriety that with a large number of gynecologists on the other side of the Channel, remedies of a purely mechanical nature seem at the present day to find most favour. The agencies chiefly relied on by them for producing a sanative effect upon the uterus, are such as exert a *direct physical* influence, and are selected with this very intention.

Is it not on this ground, and none other, that incision of the cervix uteri is so frequently had recourse to for dysmenorrhœa and sterility, being employed by some practitioners with a frequency that is perfectly astonishing, and also with an occasional fatality that is not at all astonishing. "The whole philosophy of this operation (writes one of its ablest advocates) consists in opening the canal, and keeping it open, so as to allow the easy passage of the menstrual flow." He is speaking, you perceive, of dysmenorrhœa, of which he tells us in another page that "it is only a symptom of disease, which may be inflammation of the cervical mucous membrane, retroflexion, anteflexion, fibroid tumour in one wall of the uterus or the other, contraction of the os internum or os externum, flexures of the canal of the cervix, either acute or gently curved, either at the os internum, at the insertion of the vagina, or extending through the whole length of the canal, all of which are but so many *mechanical causes of obstruction*, which must be recognized and remedied if we expect to cure the dysmenorrhœa"—(Sims, p. 143). And again:—"I am fully of the opinion that it (dysmenorrhœa) is simply a sign or symptom of disease to be found in some abnormal organic condition. This may be inflammation, or it may be the cause of inflammation, or it may exist without it. But

whether inflammatory or not, its *action is mechanical*. I lay it down as an axiom that there can be no dysmenorrhœa, properly speaking, if the canal of the neck of the womb be straight and large enough to permit the free passage of the menstrual blood—in other words, that there must be some *mechanical obstacle* to the egress of the flow”—(p. 142).

But to resume. In the innumerable contrivances for supporting the uterus and correcting curvatures, deviations, flexions, and versions of this organ, do we not see a mechanical theory predominating above every other?

“An undue development of this mechanical tendency,” observes Dr. Tilt, “gave rise to a system of uterine orthopedics, in which the incontestable symptoms of inflammation of the womb were accounted for by its displacements.”—(*Uterine Therapeutics*, p. 169).

But perhaps the most convincing proof (though a melancholy one) of the supremacy of these intro-mechanical ideas is to be found in the keen rivalry—even among men of real ability and deserved eminence—about the priority of invention of some apparatus for dilating, or cutting, or stretching, or rectifying the uterus, or such like purpose.

If the reproductive organs of the female be governed by no other laws than those which regulate the movements of a piece of machinery, we *should* concentrate all our attention on the improvements of our mechanical appliances, and attach the highest importance to them.

But, gentlemen, I need not remind you that in addition, but superior to physical and chemical laws, are the vital laws which exercise their influence as certainly, as powerfully, and as constantly upon the sexual organs in health and disease as on any other part of the living organism.

I repeat, then, that in the etiology of uterine diseases, and in the search after remedies, the minds of many able and zealous modern enquirers seem to have taken too much of a mechanical direction. They have evidently fallen into the great error of the iatro-mathematical school, in ignoring or not sufficiently recognizing the dynamic, vital properties, and conditions of the organs concerned.

Whilst pressing this charge I freely acknowledge that some progress has been made in gynecology under the influence of doctrines which are now, as appears to me, being pushed to an unphysiological extent. In the same manner, Borelli and the iatro-mechanists of the seventeenth century may be said to have rendered some service to physicians and the general advance of medicine, though by over-estimating the importance of their doctrines they committed a grievous error, and justly incurred the charge of medical heresy.^a

^aThe above remarks, just as they now appear, were written some months ago. Within the last few days my attention has been directed, for the first time, to a paper, by Dr. Tilt, on Extreme Surgical Tendencies of Uterine Pathologists, and on The Division of the Cervix Uteri, read before the London Obstetrical Society. I am glad to see that my opinions are, to a great extent, shared in by him and other gynecologists of great ability and large experience. *Magna est veritas.*

In medicine, then, as well as in other important matters, we have need to keep in mind the maxim of the Roman satirist—

“Est modus in rebus, sunt certi denique fines,
Quos ultra citraque nequit consistere rectum.”

With regard to the employment by the physician of mechanical agencies, a wide distinction must be drawn between diagnosis and therapeia.

For the purposes of diagnosis, I believe we can hardly over-estimate the importance of physical means, as with their assistance we can give more certainty to the evidence of our senses, and multiply the data whereon to base our conclusions.

But in the matter of therapeutics the case is quite different. Here the recognition of vital properties and physiological laws should teach us that all remedial measures whose action is plainly mechanical, and which are, therefore, incapable of affecting the vital condition of the organs concerned, must necessarily have a very circumscribed range of utility.—
10th November, 1866.

MR. COLLIS read a paper *On Hare-lip*.

DR. ATTHILL read a paper *On Intra-uterine Polypi*, and exhibited *A New Form of Ecraseur* (see p. 60).—December 8th, 1866.

On Rigid Perineum. By DR. BEATTY.

The management of the last part of the second stage of labour is often attended with difficulties that demand the utmost care, and are productive of serious anxieties in the mind of the individual charged with the conduct of the case. This remark applies more particularly to the phenomena of parturition in primipara; though at times circumstances of a similar nature are found to create embarrassment in those who have already borne a child at the full period.

All persons who have been any time engaged in the practice of midwifery are well acquainted with the tantalizing torments of a rigid perineum. Hour after hour the attendant sits by the bedside; every pain distending the soft parts seems destined to be the last; the structures, strained to the utmost, seem incapable of further resistance, yet they do resist, until finally a rent at the fourchette takes place, most commonly to a small extent, sometimes to a more considerable one, and the head of the child escapes from the pelvis. In many cases of moderate rigidity, the delivery is accomplished without any rent; but in the more obstinate cases, the greatest amount of care, exercised by the most skilful hand, will fail to prevent some amount of laceration. A knowledge of this fact should lead us to be very cautious in dealing with the reputation of the attendant who has had the misfortune of having such a case under his charge. Every man in practice is likely to encounter such cases, and in some of them no man can prevent the accident. In speaking of this subject, Dr. Denman makes the following remarks:—

“That some degree of laceration should sometimes occur will not be surprising, if we consider the great change and violence which all those parts sustain at the time when the head of the child is passing through them, or that when a laceration begins it should extend through a part rendered at that time extremely thin, and suffering an equal degree of force. When the perineum is indisposed to distend, or if when distended it cannot permit the head of the child to pass with facility, the anterior part of the rectum is dragged out, and gives to the perineum a temporary elongation. The true perineum, and the temporary, as it may be called, thus forming an equal uninterrupted space; if a laceration should commence at any part it might, without the greatest care, extend through the whole.

“That kind of laceration of the perineum which commences at the anterior edge, and runs obliquely or directly backwards, is alluded to in every dissertation on this subject. But there have been many instances of another kind of laceration, which may be called a bursting or perforation of the perineum at that part which is connected with the circumference of the anus, when the anterior part is preserved, and through such perforation, it is said, children have been sometimes expelled.”

A remarkable case of this kind occurred in the practice of the late Dr. Beatty, in the year 1808, from whose case book I now quote it:—“October 17th, 1808. I saw this patient in labour with her first child, about seven o'clock in the evening, after having had slight pains during the day with very little effect on the os uteri. I saw her again 10½ p.m., when the progress of labour appeared to be slow. While I remained with her the pains became more frequent, and in a very short time the head rested on the perineum; but what appeared strange to me was, that though the pains continued to be very severe, and the tumour caused by the head distending the perineum to increase, there was not the slightest dilatation of the os externum beyond its original size. In about an hour the head of the child was entirely expelled from the bony pelvis, and the external parts formed a bag or cap for it, which was forcibly distended at every pain. My fears of a laceration now increased so much that I thought it necessary to explain them to an intelligent woman who was with us, and to make her examine the parts, that she might be convinced of the impossibility of preventing it; at the same time I used lubricants to satisfy the friends that I would do everything in my power for my patient. At length I found the perineum begin to chip or crack at the prominent part, and soon after give way to such an extent that the child was passed through the aperture, though it did not communicate with the os externum. The placenta was delivered through the same passage; and when I told the lady that she had had the most painful labour I had ever met with, she said she expected such, from a contraction which had taken place in those parts when she was young, after a fever, a contraction which had almost prohibited coition.

“The os externum had left an oval mark on the child's head, which I

measured, and found to be $2\frac{1}{2}$ by $1\frac{3}{4}$ inches, and which was the full extent to which the vulva would yield.

“October 28th—this day examined the state of the parts, and found both the sphincters of vagina and anus entire and undisturbed, and the rectum uninjured. The patient was able to walk a little through her room. The wound was in a healthy state, and likely to heal.”

But such an accident as this is not the worst that occurs under similar circumstances. A more frequent result is the extension of the rent, commencing in the middle and most prominent part of the distended perineum, and its prolongation through the sphincter ani behind, and the vulva before, thus throwing the two passages into one, and entailing the misery of uncontrolled defecation upon the unfortunate patient.

As I have already said, no amount of the most careful attention on the part of the medical attendant can prevent some of these extensive lacerations at times. The split will begin either in front, and run back to the anus or into it; or it may begin in the middle and embrace both sphincters before it stops; or the head may come through the rent in the middle, and leave both sphincters untorn. In whichever of these ways the struggle terminates the result is very calamitous, and entails a vast amount of suffering on the patient and of trouble to the attendant in whose hands the accident has occurred, to whom the patient and her friends will attribute all the blame. These cases are, in the present day, not so lamentable as they were formerly, for plastic surgery and wire sutures enable us now to remedy the evils in a manner that would astonish our forefathers. I look back with regret to the case of a most interesting, very handsome patient, twenty-five years of age, the wife of a cavalry officer, that passed through my hands thirty years ago. I was engaged to attend her with her second child, and when her labour began I was surprised to find the perineum split into the rectum. I learned that this had happened in her first confinement, which took place before she came to this country, and, as usual, the strongest invectives were heaped upon the head of the unfortunate doctor who had attended her. Her second labour, I need not say, was easy enough, and I saw her for a long time after her recovery. But her life was miserable; she could not venture into society, for she was unable to control the passage of feces or of flatus from the bowels, and she never knew when one or other would escape. If I then knew and could have done what I now know and can do, I would have been able to restore that young creature to health and comfort, and to that position in society which she was intended to adorn. I think it is extremely probable that cases like this are more numerous than is generally believed. Until very recently, it was too well-known that nothing could be done to relieve them, and unfortunate sufferers bore their misery in silence, not wishing, naturally enough, to make known their infirmity when of such a disgusting and

incurable nature. Since the means of remedying the evil have been devised and successfully practised, the number of cases that seek relief has wonderfully increased ; and in the last edition of Mr. Baker Brown's work on the surgical diseases of women, he gives the details of no less than 112 cases in which he has operated. When such a vast number has fallen to the lot of one surgeon, we may have some idea of the multitude that must be scattered over the empire.

Seeing, then, that extensive laceration of the perineum is of sufficiently frequent occurrence to arrest the attention ; and knowing, as we do, that at times the most skilful care, as at present practised, is impotent to avert the calamity, it behoves us to enquire more particularly into the nature of such cases, and ascertain the cause of the accident, with the view of discovering some means of preventing it, more effectual than those in ordinary use. When we look back on our own experience, and read the accounts of such cases in authors who have treated the subject, we find that the conditions of the parts for some time preceding the rupture is as follows:—The head of the child has escaped from all bony resistance, and is well out of the pelvis, carrying the perineum and anterior wall of the rectum before it, these parts forming a cap or bag in which the head lies. The vulva, however, remains undilated, the efforts of the uterus seem unavailing to cause any extension of that opening, and the head, which in the earlier stage of this part of the process had been driven against it, has now, by the yielding of the perineum, sunk below its level ; and every pain drives it lower, so that all the force is expended in an endeavour to tear through the bag in which the head is embraced. The soft parts finally give way, often in the middle, most prominent part, and the rupture is effected. The term, rigid perineum is, in fact, hardly applicable to these cases ; it is the vulva that is rigid, and its resistance that causes the mischief. It is unyielding of the vulva that is the immediate cause of the danger.

Let us now turn to nature's book, and enquire what means she adopts to escape from the difficulty in cases somewhat less exaggerated than those I have just described. In minor degrees of rigid vulva the head is enabled to take a more forward course, a greater amount of it is permitted to emerge at each pain, but the tissues will not or cannot yield sufficiently to permit the head to pass through ; the opening must be enlarged before delivery takes place, and accordingly in a vast number of such cases, it is notorious that some fibres of the fourchette give way, and immediately the head is expelled. A great deal may be done by carefully supporting the perineum to prevent this rent going too far, but no amount of care will prevent some laceration in many of these cases. Nature I believe to be a very good doctor, and often accomplishes her ends better than the best of us, and moreover, often points out to us the right way to help her out of difficulties, if we study her proceedings, and are not too proud or too timid to imitate her.

The following case will show what can be done by taking nature for a guide:—

On the 14th of November, 1866, a remarkably fine, well-made lady, twenty-five years of age, and just nine months married, took labour at 2 o'clock, a.m. I was sent for, and saw her at 8 o'clock, a.m., at which time the head of the child was well down through the pelvis and nearly rested on the perineum. The head was covered by the still undilated uterus, the os uteri being the size of a two-shilling piece. The vulva was very small, but did not then seem rigid. The pains were natural, the os slowly dilated under their influence, and in another hour, at 9 a.m., it was fully open, and the head distended the perineum at every pain. I looked forward to a speedy delivery, and took my place by the bedside. The head came lower and lower, pushing the distended perineum before it, and at each pain a small portion of a very hairy scalp was protruded through the vulva. In this position I remained from 9 a.m. to 2 o'clock p.m., just five hours, during which time the pains were increasing and more violent, distending the perineum to a frightful extent. I wished to give her chloroform, but she refused to have it. The perineum seemed like a bag into which the head was driven with every intention to tear through it. The soft parts over the head from the edge of the anus to the fourchette, measured nearly $3\frac{1}{2}$ inches; the anus was distended at every pain, showing fully an inch and half of the interior of the rectum. During the whole of this time there was no further dilatation of the vulva, and no advance of the head forwards; the whole force of the uterus seemed to be directed towards driving the head either through the perineum or through the rectum. It became quite manifest that one or other of these must occur, for there did not appear to be the least chance of the head escaping through the vulva. When matters were in this state at the end of five hours most desperate struggle, I argued with myself, that if nature so often puts an end to such difficulty by the yielding of the fourchette and anterior fibres of the perineum, the best way to rescue my patient from the frightful laceration that was so impending would be to imitate nature, and enlarge the opening of the vulva. Accordingly, seizing the moment when a furious pain, that almost drove the head through everything had subsided, I introduced one blade of a probe-pointed scissors between the perineum and the head, and divided an inch of the soft parts. The very next pain passed the head out through the vulva with the greatest ease, without a single fibre being torn, or the slightest extension of the opening I had made. The recovery was perfect. No treatment beyond ordinary washing was adopted, for the wound, which healed spontaneously, so that the nurse in attendance remarked to me some days after, that I ought to have taken measures to prevent it healing so well, for there would be the same trouble at her next labour. By this simple imitation of what nature so often does I terminated a most difficult and perilous labour

without the slightest ill result to the patient. I find, in two recent authors, allusions to an operation similar to that which I have just described. Doctor Hall Davis, in his very excellent work,^a says:—"In organic or structural rigidities due to hard cicatrices from former sloughings, sometimes depending upon plastic operations extended too far forward to admit of the exit of the head, these means (chloroform, warm fomentations, unctuous applications, and warm water enemata) may fail. In two cases, last year under my care, such cicatrices were the obstacles, and not yielding to chloroform, in one a rent was inevitable, which, however, left an adequate perineum behind. In the other case, rupture being expected every moment, I summoned the surgeon who had operated, and suggested his making a slight bilateral incision downwards, and outwards to the extent of a quarter of an inch. This sufficed, and the child, living, immediately passed out without any extension of the incisions, which had healed in two days afterwards; thus the perineum was saved." In the last edition of the highly valuable work of Mr. Baker Brown,^b he observes:—"In cases where rupture seems inevitable during delivery, Dr. Blundell recommended and practised the plan of relieving the tension of the perineum by a slight lateral or oblique incision during a pain, thus actually producing a laceration, but one of no moment, if it serve as intended, to prevent the tear along the median line, where it naturally takes place, and proves of serious consequence. This plan I concur with, and would practise when chloroform failed or could not be administered. MM. Paul Dubois and Chailly-Honoré advocate an oblique incision of the vulva towards the perineum about a third of an inch long, either to prevent altogether the rupture of that region when much distended, or when the laceration is unavoidable, to favour it at a spot where it may produce the least mischief. The writers support their views by the history of successful cases." Since reading the above quotation I have searched through Dr. Blundell's work, and not finding any allusions to the operation in question, I inquired from Mr. Baker Brown, and he has kindly informed me, that he attended Dr. Blundell's lectures, and heard him advocate the proceeding.

I am quite sure that none of the members of the society will imagine that I undervalue the well-known means so advantageously employed to induce relaxation of the perineum and vulva, such as bleeding, antimony, chloroform, warm fomentations, and lubricants, or the protection to be obtained by careful support of the perineum; my object in this communication is to impress upon them, that in extreme cases, such as I have described, after all ordinary means have failed, and frightful injury is impending, a simple operation in imitation of what nature does will avert the danger, and place the patient and her offspring in safety.—12th January, 1867.

^a Parturition and its Difficulties.—P. 13. 1865.

^b Surgical Diseases of Women. 1866. P. 10.

On Cephalotripsy. By DR. KIDD.

Cephalotripsy, or the breaking down of the bones of the head, previous to extracting it from a narrow pelvis, is spoken of in German, French, and American works on midwifery, as an operation of the greatest usefulness and highest importance, an improvement, in fact, in scientific obstetrics only second in value to the introduction of the forceps, yet in our standard English works it is ignored entirely, or spoken of as an operation that could not, and has not, been attempted in this country. This repudiation of the operation may be traced to two causes—the formidable appearance of the instrument first recommended for its performance; and the mistaken idea that it should serve as a substitute for perforation of the head and evacuation of its contents, instead of these being essential stages in the operation.

We are indebted to Sir James Simpson for a great improvement in the construction of the instrument; and though he has not published any account of it, so far as I am aware, I know from personal communication that he uses it in all the suitable cases he meets with, and that it is daily growing more and more in favour with him. It appears also from the records of the London Obstetrical Society that Braxton Hicks, Greenhalgh, Graily Hewitt, and others, are beginning to adopt it in their practice. I have myself had Sir James Simpson's cephalotribe for some time, and used it in such cases as required it, and am convinced of the correctness of the principle on which the modern operation of cephalotripsy is founded, and of the suitability of the instrument we now possess for its performance, and I bring the subject before the Obstetrical Society because I think it full time that the merits of the operation should be fully discussed in a society that so fairly represents the Irish school of midwifery.

The earliest cephalotribe known to us was invented by Assalini, and described in a work published at Milan in 1811; but till Baudelocque, nephew of the celebrated accoucheur of the same name, invented his instrument in 1833, the operation of cephalotripsy seems to have been but little practised. Since then the value attached to the suggestion has been evidenced by the very many attempts to improve the instrument. At the Exhibition of the London Obstetrical Society last year the following seventeen different forms of it were exhibited, besides three varieties of chain-saw forceps, an instrument that may fairly be classed with it; and Hodge's, and probable other forms were not represented:—Assalini, of Milan; Lazarewitch, of Charkoff; Baudelocque, Paris; Baudelocque, by Luer; Depaul, Paris; Depaul, by Charrière; Etlinger and Hugenberger, St. Petersburg; Martin, Berlin; Braun, Vienna; Nyrop, Copenhagen; Cohen, Hamburg; Rizzoli, Bologna; Hennig, Leipzig; Scanzoni, Würzburg; Killian, Bonn; Sir James Simpson, Edinburgh; Schöller.

And of chain saw forceps:—Van Huevel, Brussels; Faye and Mette, Christiania; Lazatti Milan.

The accouchers of repute who have advocated the use of the instrument are very numerous. I may mention Chailly, Cazeaux, Dubois, Leynseele, Scanzoni, Killian, Cr    , Kiwisch, Hodge, Bedford, and Pajot. Chailly says it has completely banished from obstetric practice crotchets and craniotomy forceps, and all such hooks and pincers. Dr. Hodge, Emeritus Professor of Obstetrics in the University of Pennsylvania, in his great quarto volume on the principles and practice of obstetrics, describes the introduction of the cephalotribe as an improvement in scientific obstetrics only second in importance to that of the forceps. In speaking of the dangers of craniotomy, he says:—

“The slightest consideration of the *modus operandi* of crochets, pincers, craniotomy forceps, and other varieties of tractors, demonstrates that they act not directly, but indirectly, in accomplishing the diminution of the head of the child, so that it may pass through the contracted passages. The head is in reality lessened—not by the tractors, but by the bones and soft tissues of the mother; for it is dragged through, for example, the superior strait of the pelvis, and this is the real agent which diminishes the head. The bony strait is covered by delicate and important tissues, such as the edges of the uterus, the vagina, bladder, &c. Hence such tissues are powerfully compressed between the bones on one side, and the head upon the other, greatly endangering their integrity and safety. The greater, therefore, the contraction the greater will be the risk to the tissues. No wonder then that there is danger of contusion, laceration, inflammation, ulceration, and mortification of these tissues in bad craniotomy cases. Hence accoucheurs have always dreaded craniotomy operations in confined pelves, as they have too often proved fatal, not only to the integrity of the bladder, rectum, and other tissues of the pelvis, but also to the life of the mother.”

The principle of cephalotripsy, according to Dr. Hodge, is that the head of the child should be reduced in size, by compressors, and not by the tissues of the parent, as in all the usual operations by the crotchet and craniotomy forceps:—“The importance of the principle now inculcated can hardly,” he says, “be over estimated. Next to the introduction of the forceps into modern practice, the idea, when the preservation of the child’s life is hopeless, of diminishing the size of the head by compressors, instead of dragging it through the contracted outlets of the body by mere force, to the great detriment and often destruction of the mother’s tissues, seems one of the most important improvements in scientific obstetrics.”

Scanzoni, in his work, advocates the operation not less warmly. In the section on the influence of perforation on the life and health of the mother, he states that the fatality of the operation and the sad results

that so frequently follow it, are not due to the perforation, but are the results of the efforts necessary for the extraction of the perforated head. We are convinced, he says, that the results will be more satisfactory for the future if a suitable perforator (he recommends a trepan) be used, and the extraction be made with a properly constructed cephalotribe.

He further speaks of the usefulness of the cephalotribe for diminishing the head and extracting it; and in a section devoted to a description of the instrument, gives the following rules for its use:—

“1. The cephalotribe is necessary when, after perforation, the head is not expelled (the necessity for previous perforation was already insisted on).

“2. Recourse should be had to the cephalotribe without previous perforation (which would then be impossible) when, the infant being dead, the inferior members and trunk have been expelled and the forceps cannot be applied to the head, which is above the superior strait, or in the pelvis, and cannot be extracted by manual efforts. By means of the cephalotribe the head can be seized firmly, reduced in volume, and delivered promptly without injury to the mother.

“3. It may be used to extract the head when it has been separated from the trunk, and is engaged in the pelvis.

“4. This instrument may be employed under the following circumstances to seize different parts of a dead fetus:—

“a. To extract the breach of a dead child when there is a difficulty in its passage through the pelvis, or its passage endangers the safety of the mother.

“b. To compress and extract the shoulder when it is delayed after the head has been disengaged, when other methods have failed, and the diminution of the volume of the thorax is indispensable; the cephalotribe is greatly to be preferred in this case to the use of cutting or pointed instruments which can wound the uterus.

“c. To diminish the volume of the thorax, after the expulsion of the lower extremities, when it is sufficiently large to prevent the extraction of the arms which are turned upwards.

“d. Finally this instrument may be recommended in presentation of the trunk, when the thorax is so engaged in the pelvis that it is impossible to introduce the hand into the uterus to perform version—the chest may be broken and a passage so cleared into the uterus.”

“Conditions of operation:—

“1. It is necessary that the pelvis should have dimensions sufficient to allow the broken fetus to pass when we wish to extract with the cephalotribe. If it be less than $0^m\cdot068$ (2·69 English inches) in its shortest diameter the operation cannot be performed without exposing the mother to grave risks, if the child be mature, and no accoucheur

should attempt it. It may be tried when the fetus is not fully developed, and is of small size, though the pelvis is not more than 0^m·055 (2·16 English inch) in its shortest diameter.

“2. The os must be sufficiently dilated.

“3. The head must be fixed in the brim of the pelvis, or so placed that it may be fixed at the brim by pressure applied to the abdominal walls.”

The objections that have been urged against the cephalotribe are chiefly as follows:—

1st. The imperfection of the instrument. 2nd. That while it diminishes the diameter of the head in one direction it increases it in the opposite. 3rd. That in comminuting the bones it causes splinters, which may protrude through the skin and injure the soft parts of the mother.

Dr. Churchill, in the fourth edition of his *Midwifery*, says the appearance of the instrument is so formidable that he doubts if it could be used in this country, and he is not aware that the attempt has been made.

This refers to Baudelocque's cephalotribe, and speaking of the same instrument, Dr. Murphy says:—“Look at it and ask yourselves how could it be used in Elizabeth Sherwood's case—to me it seems impossible.” Dr. Tyler Smith makes a somewhat similar objection. In no other English class book that I am aware of is the cephalotribe even mentioned.

A comparison of Sir James Simpson's cephalotribe with some of those that preceded it will sufficiently remove the objections to the instrument on account of its enormous proportions:—

Measurements of Cephalotribes.

	Weight	Entire Length	Blades					Handles		Power
			Length	Breadth	Thickness	Breadth Closed ^a	Pelvic curve ^b	Length of Handles	Length of Joint	
Baudelocque,	4 12 lbs. oz.	20½ In.	10½ In.	1½ In.	—	1½ In.	—	—	—	Winch and screw
Hodge,	3¾ lbs.	19½	6½ Shank × 3½	1½	—	1½	3	9½	—	Screw and fly-nut.
			9½			Curve for Head				
Scanzoni,	—	18·66	10·15	0·98	0·27	2·04	3·81	8·54	—	Screw and lever, with travelling bar.
Braun,	—	16	8	0½	0·2½	2·2½	—	6½	1½	Screw and joint in handle.
Simpson,	2¼	14	8½	1½	—	2	2½	4½	1	Screw and fly-nut.

^a Breadth of blades closed is measured across the blades when locked and closed, and includes the thickness of the blades except in Scanzoni's.

^b The pelvic curve is measured by placing the instrument on a horizontal plane and measuring the length of a line falling perpendicularly from the highest point of the blade to that plane.

The second objection that I have mentioned, that all the diameters of the head are exaggerated except that in which the instrument is applied has been examined by Hersent by experiments made on the heads of twenty dead children. In five instances in which the cephalotribe was applied, without the head having been perforated and emptied of its contents, he found that the diameter in which the blades were applied was diminished, while all the other dimensions were exaggerated. In the other instances, in which the head had been perforated and emptied of its contents, all the dimensions were diminished.

These experiments were repeated by Hodge, but he does not seem to have perforated and emptied the cranial contents in the first place. The transverse diameter, where the blades were applied was easily reduced, he says, to two inches, while the occipito-mental diameter and the cervicobregmatic were lengthened.

Dr. Hodge also found that the bones were turned inwards upon the cavity of the cranium, and though fractured, in no case did they penetrate the scalp, even when no perforation had been previously made.

The head that I exhibit is one on which I operated with the perforator and cephalotribe, in the month of June last; and on inspection it will be seen that the bones collapsed and turned inwards under the pressure of the blades, and that while the transverse diameter was shortened there was no, or scarcely any, lengthening of the other dimensions.

I have now shown, so far as authority can do so, that the operation of cephalotripsy is worthy of more consideration than it has hitherto received in the Dublin school. My own experience of the cephalotribe, as a means of extracting the head after it has been perforated, and the brain has been broken down, that is, of lessening the size of the head and of drawing it through a narrow pelvis, leads me to regard it as a most admirable instrument. By the use of it the danger of craniotomy may be reduced to a minimum, and in many cases delivery safely accomplished, in which but for it we would be called on to recommend the Cesarean section. Not that I think it can ever supersede this operation altogether, but it will lessen the number of the cases in which the necessity for this last resource of our art will arise.

Of cephalotripsy without the previous use of the perforator I have no experience, nor do I think that it should ever be resorted to in head presentations till the contents of the cranium have been broken down.

I have now used the cephalotribe in three operations. The first was the case in which the head just referred to was removed. It was the mother's first labour—the pelvis was small in all its diameters—the head lay in the brim in the third position of Naegele, Dr. Sinclair and Professor Haughton were present at the operation. Dr. Sinclair and I first tried to deliver her with the forceps, which was easily applied, but by no effort that we thought it prudent to make could we move the head

from its position. The fetal heart had not been heard, though we searched for it carefully, and as the mother's condition demanded relief, I perforated, and applied the cephalotribe, and extracted the head with great ease. The woman made a rapid recovery, and left the hospital on the ninth day.

The second case was that of a woman living in a court off Cuffe-street. She had been delivered by craniotomy four times previously; once by myself, once by Dr. Sawyer, by Dr. Ringland, and by Dr. Mason. I was called to see her on Saturday, 24th November. She had been in labour from Wednesday morning. It was a head and arm presentation; the promontory of the sacrum projected very much forwards—the brim was under three inches in its antero-posterior diameter, and the head was completely above the brim, being prevented by the arm from becoming in any way engaged in it. The fetal heart was inaudible. The mother was in a state approaching collapse. So tender was the abdomen that she could not bear the slightest touch; she had constant vomiting, the surface was cold, the pulse almost imperceptible, and the uterine contractions had ceased for some hours. In the absence of my colleagues, Drs. Ringland and Sawyer, Dr. Churchill kindly saw her with me; and I was very glad to have his co-operation, for while it was imperative that the woman should be delivered immediately, I feared she would die under the operation. With Dr. Churchill's assistance, I perforated and broke up the brain, and then applied the cephalotribe. The head was so movable, from being completely above the brim, that I attempted to fix it with the crotchet, while introducing the cephalotribe, but I found this interfered with the proper application of the blades, causing them to slip when I began to extract, so I removed the crotchet and reapplied the cephalotribe, while Dr. Churchill steadied the head by applying his hand on the abdomen of the mother. The head was easily seized and compressed, and was extracted with a degree of ease that surpassed my expectations. However, the operation had been too long delayed, the woman never rallied, and died in about eighteen hours. The ease with which the head was extracted in this case was very remarkable. Under the extractive efforts it rotated in the pelvis so as to bring the shortened diameter into the narrowest part of the pelvis, and I have little doubt that the efforts that would have been necessary to extract it with the crotchet would have caused the woman to die under the operation.

The third and last case in which I have used the cephalotribe (for, fortunately craniotomy is not required as frequently now as when the use of the forceps was less known) occurred on the 28th December last. It was the woman's second labour; she had been delivered previously in the Rotundo hospital, by Dr. Cronyn, by craniotomy, and her recovery was retarded by the occurrence of pelvic cellulitis, which prevented her leaving the hospital for six months. When I saw her I found the os very nearly

fully dilated, but still presenting a rigid band all round it. The head, which presented, seemed to be hydrocephalic, and one of the parietal bones had passed through the os, while the remainder of the head was above the brim, which was very much encroached on by the promontory of the sacrum; labour had commenced three days previously, by early rupture of the membranes. The woman was flushed, skin hot, belly tender, and she had vomiting—fetal heart not to be heard. Dr. Cronyn kindly saw this woman with me, and recognized her as having been the subject of a very difficult and tedious operation by the crotchet. We at once perforated and applied the cephalotribe; on making the first effort to extract I experienced some difficulty, so I unscrewed the instrument, and applying it in another direction, gave the bones another squeeze, when I was enabled to extract the head without further difficulty. The shoulders were now caught at the brim, and Dr. Cronyn, who kindly completed the delivery for me, as I was suffering from a severe attack of influenza at the time, had very great difficulty in extracting them—so much so, that we thought of applying the cephalotribe to break down the thorax—but after some time he completed the delivery without this.

The next day the pulse was down to 90, and soon came to the normal standard, and the woman made an excellent recovery, notwithstanding that a portion of the lip of the uterus was found, after delivery, to be almost entirely detached and hanging in the vagina. How this portion of the lip was injured I really cannot say; it was certainly not done by the cephalotribe, because it was the posterior lip, and the blades were applied laterally. I incline to think it was caused by the long-continued pressure between the head of the child and the promontory of the sacrum, which was very sharp and projecting.

These cases have fully convinced me of the superiority of the cephalotribe over all other methods of extracting the head after it has been perforated. In two respects, I think Sir James Simpson's instrument capable of improvement. The pelvic curve, which is less in his than in most others, might, I think, be done away with altogether with advantage. In cases of antero-posterior narrowing, it is desirable to turn the shortened diameter of the head into the short diameter of the pelvis, and the space through which the head will have to rotate when turned with a straight instrument will be considerably less than when turned with a curved one—besides, the introduction of a straight blade is easier and safer than that of a curved one.

The lock might also, I think, be reversed with advantage. That is made so that the lock of the upper or right blade will look forwards when the blade is introduced. This would, I think, be an improvement in the construction of all midwifery forceps, as well as the cephalotribe. I have a forceps of very old date made in this way; and in the museum of the Rotundo Hospital there is one labelled as Denman's, made in the same

way. Why all our modern instruments are made differently I cannot tell. The present construction obliges us to adopt one of three courses :—*a.* Follow the rule laid down by Rigby, and introduce “ the upper or lower blade first, according as its lock is directed forwards ; *b.* Introduce the second blade behind the first ; *c.* Introduce it in front of the first, and afterwards cross the handles so as to bring the locks opposite to one another. Each of these courses is objectionable. The first, because it implies, as instruments are at present constructed, the introduction of the lower blade first. The second, because it interferes with the proper placing of the instrument when the head is high up, as it is then necessary to carry the blade as far backwards as the perineum will permit, that the blade may be properly placed on the head, and that the first tractions may be made in the axis of the brim of the pelvis. In the third method there is some risk in crossing the handles of displacing the blades however well they may have been applied, and in a narrow pelvis the crossing of the handles may not be easily accomplished. By making the instruments so that the lock on the upper blade will, when it is introduced, look forward, the lower blade can be introduced in front of the other, and the locks will come properly into position ; and while the introduction of the instruments is thus much simplified their utility is in no way impaired. I have had a cephalotribe made for me by Messrs. Fannin, resembling Sir James Simpson’s, but without the pelvic curve and with the reversed lock, and I believe that with these alterations it is a very perfect instrument.

In using the cephalotribe when the head presents perforation is performed in the ordinary manner, and the brain carefully broken up. Then the blades of the cephalotribe are introduced in the same way as the forceps, but care must be taken to pass them up sufficiently high to grasp the base of the skull, as the great object of the operation is to break it, and reduce its size. For this reason the points of the instrument, especially when the head is high up, must be passed well forwards, and the handles must be carried back as far as the perineum will admit, and the reversed form of the lock will now be found specially useful. When the blades are locked the screw is passed through the openings in the handles, and by screwing the handles together we crush the head. This being done, we now proceed to extract it, when, if any difficulty be experienced, the head may be rotated so as to bring it, with its shortened diameter, to the narrowest part of the pelvis ; and here the advantage of having an instrument, without the pelvic curve, will be experienced. If there still be difficulty the blades may be applied to the head in another direction, and the base of the skull again broken ; and this may be repeated if necessary, and then extraction performed as if with the forceps.

In cases of extreme narrowing of the pelvis, Professor Pajot^a, of Paris, operates by repeated crushings without tractions, a method that he recommends when the pelvis measures under six and a half centimètres (2·5 English inches), and which he says may be practised where it is not more than 27 millimètres (1·06 English inches), the smallest size that will admit the introduction of the instrument.

As soon as the os is sufficiently dilated he perforates; and when it is large enough to allow of the introduction of the cephalotribe, he effects the first crushing, taking care to grasp the base of the skull. Then he slightly rotates the head to the right or left as he finds most easy, but does not persist in his efforts to do this if he finds much difficulty, as the uterus itself will, in a little time, and often in a very short time, rotate the head so as to bring its diminished dimensions into the narrow part of the pelvis. After the first crushing he withdraws the instrument without traction, and proceeds immediately to make a second and even third crushing, and then the patient returns to bed. According to the general and local state of the patient, and the weakness or energy of the uterine contractions, he repeats these crushings every second, third, or fourth hour, making two or three crushings at each sitting; in some cases one or two sittings suffice. The head, broken and elongated, is expelled by the uterine contractions; and if the thorax present any difficulty it also is crushed once or twice.

M. Pajot recites seven cases where this method was practised, the leading features of which I have condensed into the following table:—

Pelvis measured	English inches	No. of Sitzings	Head expelled	In about
6 C. M. = 2·36.	Successful.	2.	(spontaneously)	24 hours after perforation.
5 C. M. = 1·9.	Fatal.	2.	"	14 "
6 C. M. = 2·36.	Successful.	1.	(slight traction)	— "
6 C. M. = 2·36.	Do.	1.	(spontaneously)	5 "
5 C. M. = 1·9.	Do.	4.	"	13 "
6 C. M. = 2·36.	Do.	2.	"	8 "
36 M. M. = 1·41.	Fatal.	Cephalotripsy attempted in three sittings with an imperfect instrument before M. P. saw the patient; he operated once, but the patient died undelivered two hours afterwards—uterus ruptured.		

Professor Van Leynseele^b says truly of this method that it is only applicable to cases where the operation is determined on and commenced before labour has made much advance, or the woman suffered any fatigue, or the soft parts any compression; but in the majority of cases,

^a Archives Générales, Mar. 1863.

^b Résumé du Cours d'Accouchements donné à l'Université de Gand.

where cephalotripsy is required, the condition of the mother would not permit of any such delay and prolonged and repeated operations. I believe, however, the method is well worthy of being borne in mind, if we should meet with a case of labour in an early stage, especially if the child be dead, where the pelvis is greatly deformed, and where the decision comes to lie between embryotomy and the Cesarean section.

In conclusion, I recommend the adoption of the cephalotribe in all cases of embryotomy in preference to crotchets, hooks, and craniotomy forceps, for the following reasons:—

1st. By its use the base of the skull, the thorax, or pelvis may be completely broken up and reduced to the smallest possible dimensions without injury to the mother.

2nd. It holds the part to which it is applied so firmly that it can be rotated if necessary and extracted with ease and safety.

3rd. It causes no spiculæ, as the crotchet does, to tear the soft parts of the mother.

4th. It does not, like the crotchet, endanger the mother or the hands of the operator by slipping or perforating the part to which it is applied.

5th. It reduces, for the foregoing reasons, the dangers of embryotomy to a minimum, and allows of its performance in cases where it would not otherwise be possible.—12th January, 1867.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.^a

SESSION 1866-67.

DR. CREMEN, *President*, in the Chair.

Cholera. Illustration of the Doctrine of Contagion in Cholera, Drawn from the Epidemics of 1854 and 1856, at Mauritius. By GORDON K. HARDIE, M.D., Surgeon 73rd Regiment.

Having, twelve years ago, had my opinion on the subject of contagion in cholera completely reversed by what I saw and learned in the two great epidemics, in Mauritius, of 1854 and 1856—and having been one of the members of the committee appointed to investigate and report upon the epidemic of 1854, I think I may be able to put facts before this society which seem hardly capable of any other interpretation than the importation and transmission of cholera by means of contagion.

It is not intended, however, in discussing the question of contagion, to restrict this term to personal contact or intercourse merely. The main

^a These reports are supplied by Dr. Purcell, Secretary to the Society.

point to determine is, whether mere epidemic influences of atmospheric or telluric origin, of utterly unknown quality, facilitated by bad local sanitary conditions, are adequate to originate the disease; or whether it does not require the presence of a specific poison, originating, and capable of being reproduced, in the human body, and diffusing itself, either by emanations from the body or its excretions, or by passing into earth, air, or water, or by attaching itself to clothes or other fomites. The latter, in its various modes, just indicated, I hold to be the correct view.

In the earlier epidemics in Europe the first view was very generally held. It seems to have been thought politic by the authorities, and perhaps by medical men, to diminish panic by declaring the cholera to be non-contagious. Every successive epidemic seems to lessen the number of those holding this view, though many still attempt to limit contagion to some particular mode, of which water-contamination is at present the favourite.

I believe that a thorough knowledge and analysis of all that is known will result in proving that the poison is a most persistent one, being transmissible, directly or indirectly—remaining, with undiminished power, when hidden from sight for days and weeks, among folded clothes locked up, or sinking into the earth to contaminate the water at a later date; while its greatest and most rapid power is seen on the introduction of infected persons into a previously healthy locality, and among people never before exposed to its influence.

In India, where it is endemic, these epidemic explosions are not so easy of appreciation, and nowhere has the contagious nature of the disease been less dwelt upon or believed.

Mauritius, on the other hand, is admirably circumstanced for the study of cholera contagion. A little island in the Southern Indian Ocean, thirty-six miles in greatest length and twenty-seven in greatest breadth, belonging rather to Africa than Asia; separated, on the north, from India, by nearly thirty, *and from Calcutta, the focus of its cholera contagion, by nearly forty degrees of latitude*—with steady sea breezes, from the S.E. trade, for three-fourths of the year, with its capital town and only port of entry for foreign vessels on the LEEWARD side of the island, north-west, where yet, invariably, cholera has first made its appearance—with several races inhabiting it, of different habits and varying susceptibility to the disease—with certain localities enjoying an immunity, due, apparently, to the absence of running water—and with a sister island, Réunion, under the French flag—and with a stricter quarantine, and a less Indian immigration, escaping altogether in 1854.

The great endemic disease of the island is dysentery, which is very rarely associated with hepatic abscess, that most formidable complication of tropical dysentery, in malarious countries.

There is no local malaria in the island, intermittents being unknown among the Creoles, who have been born and bred in the island, and have not been exposed to malaria elsewhere. They are common among the Indian emigrants.

Cholera had appeared at four different periods up to 1856, when my connexion with the island ceased, with prolonged intervals between the three first epidemics.

There are no exact records of the first outbreak in 1775, but it is thus alluded to in a report to Government by a committee of British medical officers, appointed to report on the epidemic cholera raging there in 1819:

“From the reports of several individuals, some of whom belong to the medical profession, it does appear that a disease, most strongly resembling in its symptoms, progress, and termination that now under consideration did for some time prevail in the colony in the year 1775.”—(Report, 1856, p. 107.)

The second epidemic, in 1819, was universally believed by the inhabitants to have been imported by H. M.’s ship *Topaze*. Nothing has ever shaken that belief in the popular mind.

The committee, writing on this point, declared “that from a consideration of the history and progress of this disease, they feel the strongest persuasion that it is not of a contagious nature, and that it is *not of foreign introduction*.”

This finding is in conformity with a very strong hint from the governor, General Darling, who writes to them in the following strain:—

“It is unwise, if not criminal, to add to the apprehensions of the most unenlightened part of the community, who are not, on occasions of this sort, qualified to judge for themselves; and it must be necessary to point out to the committee that the estimation in which their character individually is so deservedly held, will, with proper exertions, be a means of subduing the alarm which appears to have been entertained, and which, indeed, still exists in the minds of many of the inhabitants.”

So this epidemic was dismissed, without any gain towards the solution of the question of contagion.

From 1819 to 1854, a period of 35 years, epidemic cholera seems to have been wholly absent from the island.

After the abolition of slavery, the labour question became the great problem, which was ultimately solved by the introduction of Indian immigrants on a large scale. In 1854 they formed the largest section of the population—

Indians,	-	-	-	78,388
African race.	-	-	-	48,140
Europeans and half castes,				55,348

181,876 on the 25th May,

1854.—(P. 119, Report, 1854.)

The terrible nature of the epidemic of 1854 will be at once appreciated when I state that in the month of June $\frac{1}{30}$ of the whole population was swept off by cholera, 6,035 deaths taking place from it.

The total mortality from cholera, from the 25th May to the 31st August, was 7,650.

Indians,	-	-	2,280,	out of a population of	78,388=29	per 1,000
Africans,	-	-	3,832,	"	48,140=79	"
Europeans and half						
castes	-	-	1,538,	"	55,348=28	"

The vessel alleged to have been the cause of this great calamity was the Sultany, an immigrant ship.

It left Calcutta February 14th, with 375 immigrants, and a crew of eighty Lascars. It reached Mauritius 24th March, having lost thirty passengers from cholera, which broke out thirteen days after the ship left the land heads, and seventeen after leaving Calcutta. It is most probable, from the long interval, that the outbreak had its origin in opening out some infected clothing on board. This being Dr. Clinhew's view (the provincial medical officer), who stated in evidence that several similar instances were known to him, which he had been led to attribute to this cause.

No case occurred among the crew during or after the voyage. Ordinarily, the period of first outbreak in these ships is while they are still in the river Hooghley.

The ship was put in quarantine, and under the inspection of a guard boat, at the Bell buoy, till the 30th of March, when it was ordered off to Flat Island, to land the Coolies, for thirty days' quarantine, and to return to its anchorage for ten days' quarantine of observation. But the ship did not start for Flat Island till the 7th April, and during the interval she lost five more from cholera and dysentery. She landed the immigrants on the 9th, returned to Port Louis on the 11th, and got pratique 19th April.

The immigrants improved in health so much that they were released from quarantine on the 1st May, having only lost three more—one from accident, and two from fever and debility. Their clothes were not destroyed. On the 4th May they were distributed through the island, among various estates. Of 307, one died between the 3rd May and 4th June; on five estates, where seventy-six were located, no deaths took place during the epidemic; while the total mortality in the other estates, where 238 immigrants were located, did not exceed fifty-seven among all classes.

The disease had already begun in Port Louis before these men came back, and it is, therefore, not probable that the epidemic arose from their distribution through the various districts. The origin most generally credited was, that, while the ship was at anchorage at the Bell buoy

under charge of a guard boat, the quarantine was broken by the captain going to Grand River, during the period between the 24th March and 19th April, to visit a house where two of the earliest fatal cases took place 6th and 7th May (page 7 of Report). Mr. Birichon asserted "that when the Sultany was in quarantine in front of Rochebois, he saw, almost every evening, a boat, with the word 'Monton' on the bow, communicate with the said vessel."—(P. 7).

The authorities, as in 1819, endeavoured to show that several sporadic cases had occurred prior to the arrival of the suspected ship. It might, at all times, in a large community, be easy to find cases of rapidly fatal diarrhea.

Seven were brought forward on this occasion, of which two only were fatal; one of whom had assisted at the exhumation of a corpse, returning, worn out with fatigue, December, 1854. The other was a fisherman, whose energies were daily taxed to procure food for his family. He died in February of diarrhea. The latest of the favourable cases was in the first half of March.

Between the 10th and 16th of April four cases of cholera appeared in Port Louis, of which two were fatal.

The next two were in the house said to have been visited by the captain of the Sultany, on the 6th and 7th of May. On the 12th, two more died of cholera, at Grand River, and one in Port Louis. On the 14th it broke out epidemically in the civil prison of Port Louis. Fifty-two cases, of which nineteen were fatal, occurred among 655 prisoners up to the 19th, when the prison was emptied of all but forty-five, of whom one died, and the prisoners sent to Flat Island and on board of two hulks, to which an hospital ship was attached. In the vessel the ultimate mortality from cholera was 150. Of 230 sent to Flat Island, 199 returned, August 5th. Only 303 of the prisoners ultimately returned to the prison, including those left behind.—(Page 3).

"During the latter half of May the mortality continued to increase in the town, and by the end of the month, 639 declarations had been made, of which 377 occurred between the 25th May and the end of the month, which were exclusively cholera casualties.

"By this time an universal panic had taken possession of the inhabitants of Port Louis. Most of those who had conveyance at command had fled into the country; the roads were covered with groups of fugitives, laden with their household goods, and seeking, in the purer aspects of the country districts a security they were not destined to enjoy, but into which, in many instances, they carried the germ of contagion, and, dying themselves, left a legacy of death to all around them. It is calculated that 10,000 people abandoned the town. Business was nearly at a stand still; a great part of the shops were shut.

"The streets leading to the country presented a long line of funeral processions."—(Pages 3 and 4).

It may be convenient for reference to tabulate the dates hitherto given.
February 14.—Sultany left Calcutta.

March 3.—Cholera appeared on board. Thirty deaths during voyage from it.

March 24th.—Arrival at Mauritius.

April 7th.—Left for Flat Island to land immigrants.

„ 9th.—Landed them.

„ 11th.—Returned to the Bell buoy, in quarantine.

„ 19th.—Got Pratique.

May 1st.—Immigrants released from quarantine.

„ 4th.— Do. distributed through the island.

Dates of cholera beginning in the island :—

April 10th.—Two deaths in Port Louis.

„ 16th.—Two attacks ; one death.

May 6th and 7th.—Two deaths in the house which the captain of the Sultany was said to have frequented, at Grand River.

May 12th.—Two deaths also at Grand River.

One „ in Port Louis.

„ 14th—19th.—Fifty-two cases, with nineteen deaths, in civil prison ; and disease becoming epidemic in the town.

The population of Port Louis, May 31st, was 49,000. The total mortality from the 25th May to 1st August was 3,492, an average of fifty a day, when the usual average mortality was seven daily.

The disease penetrated into all the districts of the island by the end of May, but attacked the various estates at very different periods during the epidemic. In all the districts there were estates which had a perfect immunity, a fact hardly compatible with the theory of a general epidemic influence as determining the disease.

The various corps forming the garrison were attacked as follows :—

Two cases occurred in the Artillery Barracks, Port Louis, in May.

Four „ 85th Regiment, in Port Louis, in May.

One „ 5th Fusiliers, „ June 4th.

One „ Barracks at Flacq, June 8th.

One „ „ Mahibony, June 15th.

The last barracks are particularly well placed (on a peninsula) for isolation. In both the last barracks the disease appeared later, and departed sooner, than among the surrounding population.

“The strength of the whole garrison was 1,704, of whom 56 were attacked, and 34 died = 70 per cent. of attacked. The fatal cases were 2 per cent. of the strength, which was less than half the proportion of the civil population, and two-thirds less than that of the police.

“The preventive measures taken were, thinning the barracks, putting men under canvas, or sending them to out-stations—all intercourse with the towns prohibited ; cholera belts issued ; bedding aired in the sun

every day; windows kept open day and night; and the men cautioned to report diarrhea at once."—(Report, page 8).

At this time I was quartered in the barracks of the Post of Flacq, a village and harbour on the opposite side of the island (windward) to Port Louis, and twenty-one miles distant. I was the only medical man in the place. The village had no drainage, except the natural one of the river; there were no conservancy arrangements before the epidemic broke out. The barracks and the village derived their water, for all purposes, from the river, the soldiers having to go through a part of the village, morning and evening, to bring in the water, after the cholera broke out. Except for this they were strictly confined to barracks. But supplies had to come from without, so that perfect isolation was impossible.

All was apparently well at the Post, so far as I knew, up to the 30th May. On that day, a Mr. Rénaud came down from Port Louis, with his family, ten in number, on a visit of flight to Mr. Véron, whose family numbered twelve, exclusive of seven servants. The family occupied a small one-storied house of two rooms, and outhouses, within a stone's throw from barracks.

On the 31st, I was called to see the infant child of Mr. Rénaud, ill with purging and vomiting. The child, which was at the breast, had had diarrhea before leaving Port Louis. It recovered; but another of the children, aged three, died of cholera, after forty hours' illness, on the 2nd June. From this day onward this house was a scene of constant misery and death. The child was buried on the 3rd, and the Rénauds returned to Port Louis the next day. The same day, Mrs. Véron and her son, who had both been very assiduous in their attention to the children, were both seized with cholera. She died that night; the son on the 6th. The eldest daughter was seized on the 7th; Mr. Véron was seized on the 10th, and died on the 12th. On the 14th, another daughter died; on the 15th, one of the servants; and on the 16th, an old friend, who had assisted in nursing them, died. Two servants died on the 18th, and on the 20th, another servant and child. Two more of the family were attacked, but recovered.

In all, between the 31st May and the 21st June, there were sixteen cases in this one house, of which eleven were fatal.

Contagion here showed itself in its full potency. The house, with its out-houses, was much too small for the original family; the overcrowding of ten more, laden with the germs of cholera, will adequately account for the pestilential outbreak, with its catastrophe.

No more direct proof of importation and contagion can well be adduced.

In another house, which was quite isolated, and close to the sea shore, six of the family died in twelve days, between the 6th and 18th of June:—

Father, aged 53, June 6th.
 Daughter, „ 10, „ 10th.
 Grandson, „ $\frac{1}{2}$, „ 10th.
 Mother ill, also, at the time; recovered.
 Son, - aged 14, June 12th.
 Son, - „ 18, „ 18th.
 Mother, „ 47, „ 18th.

after nursing her sick family, day and night, in spite of all warnings and remonstrances.

In another house, near the sea-side, in which sailors lived, there were seven deaths.

The second house to which I was called was that of a sailor, on the 1st June. He had returned, a few days before from Port Louis; was soon seized with cholera, and died that day, the fourth of the disease.

Though, from the 4th of June, cholera was raging, with great virulence, close to the barracks, no case occurred there until the 8th, and was thus an isolated and exceptional case. The man, who had lost all his molar teeth, was in the habit of bolting his food, without any attempt at mastication. Large masses of manioc (a kind of jam), two inches long, were passed in his stools. Diarrhea became prevalent about the 12th, and a second case of cholera occurred in a woman on the 13th; so that, with the exception of the case above, cholera was raging outside the barrack walls ten days before it got fairly into barracks. As all our supplies came from without, it will be seen that a perfect cordon was impossible; but that an imperfect cordon produced marked results in retarding the advent of disease, during the period of its greatest malignancy; for the recoveries were much more in proportion to attacks, after the first ten days.

The cholera influence was at its height in the barracks from the 14th to the 23rd June, during the last week of which, besides the cholera cases, and eight cases of diarrhea admitted, thirty-one men were treated, out of hospital, for diarrhea. Such diarrhea must be considered as an effect of the cholera contagion, operating under conditions either of diluted virus, or greater resistance to its power. Such persons have, doubtless, a considerable immunity from future risks. The general prevalence of diarrhea during epidemic cholera seems to show that the range of the disease may vary in intensity, from the most malignant form to the slightest derangement of the bowels, and that all should be viewed as one in origin, unless symptoms exist which are capable of another interpretation. Nature thus limits herself, in many cases; and, acting in imitation, we may consider the astringent opiate treatment as the rational plan for keeping the disease to its original and least-developed form.

After the epidemic, I took a census of the population, to enable me to appreciate the effect of locality upon the mortality:—

1. In the lowest level, or sea-side part of the village,
there were 21 houses, 146 occupants—24 deaths, - 16·55 per cent.
2. In the centre part, next to the barracks, there were
30 houses, with 281 occupants—27 deaths, - 9·61 „
Barracks, with 133 occupants—5 deaths, - 3·77 „
3. In the upper and more country part of the village,
27 houses, 337 occupants—7 deaths, - 2·08 „

In the barracks, and in the upper part of the village, there were either grass plots on a large scale (in the barracks), or gardens, with cultivated patches round each house, which were also more detached than at the lower parts. The good effects of grass or vegetation in fixing nascent decomposing organic matters have repeatedly struck me, and it is greatly to be desired that open spaces laid out in grass might form part of every barracks, or establishment, where large numbers are collected, as well as in the courts and back yards of the lower parts of towns. The reciprocal give and take of the vegetable and animal worlds shows how desirable it is to associate both, wherever practicable, as by parks, gardens, &c., &c.

But it is possible that, in addition to the greater crowding of the house, and the absence of purifying vegetation, the river, at its mouth, was more contaminated than at the upper part of the village.

Clothes were washed on its banks before the cholera, without any check, and it formed the only natural drainage for the village, which was on its banks.

Connected both with questions of water supply and contagion, I may instance some curious circumstances connected with a second outbreak of cholera, in a remote part of the district of Flacq, some time after the great epidemic was over.

Cholera reappeared in several parts of the district, in October—November, coincidently with a very great scarcity of water, the river being remarkably low. I was one of a committee appointed by Government to investigate the cause of this new outbreak, and drew up the report. We called attention to the coincidence of the outbreak with the great scarcity of water, but did not see how it could be associated as cause and effect. The views of Pettenkope were not then announced, but apply to much that we found and saw. He considers that, though human intercourse is essential, the soil is the chief medium, and for that end must be porous, and permeable to air and water, and must contain subsoil water, impregnated with fecal excrement; hence elevated rocky sides suffer least, while cholera spreads in hollow positions and along the course of rivers (as I shall later show was very well marked in both epidemics in Mauritius). The risk from the secondary contagious cause is consequently greatest at the time of the recession of the ground water—viz., times of drought.

No deaths from cholera were registered in the island between the 21st

September and the 16th October, 1854—the last case declared in Flacq district having taken place 30th August. One hundred and twenty-six decided cases of cholera were verified there during October and November, of which fifty-one were fatal; and it was supposed that many fatal cases were never declared. We thought it not improbable that there had been a continuity of cases in this way which had escaped registration.

On the 28th of November (printed October by mistake) we proceeded to Plaine Larcher to investigate the *first fatal case of this epidemic*, Jean Artider. His wife told he was seized at 10 a.m., 18th October, and died the same night. In the former epidemic his mother-in-law died of cholera in the same house. No one else of his family, which is numerous, has since been ill.

From the Abbé Giles we learned that cholera first began in the Camp de Masque in the beginning of July; from the 15th to the 5th August there was no apparent cessation; from that date to 2nd September many deaths; from that to 27th October two or three cases only; from that date to 10th November a great mortality, particularly in the Mare Jackos.

“At the estate of Ben Espoir we learned that there had been two cases of fatal cholera in the first epidemic in July. Had had no case after that till November, during which there were nine attacks and four deaths. There has been great want of water of late.

“At the Reve, the estate below, 130 persons were employed; no deaths in the first epidemic; ten during this month. A Creole woman, Perrine Tornitta, was the first fatal case. She had been nursing her aunt, who died of cholera, on the 4th, in the Camp de Masque; Perrine died on the 11th. She had brought her aunt's two children down to the estate with her, one of whom died, as also her husband. The water is from a canal, which has its source at Bel Etang. It at present contains very little water, and turbid for weeks from ducks paddling in it.

“At Bel Etang we went to the head of the canal which goes to the estates just mentioned. The water there was quite clear. The River Coignard, which passes down to Mare Jackos and Queen Victoria (places affected by this epidemic), also receives water from the Bel Etang. This river is very low. The manager at Bel Etang thought that imperfect sepulture had been the probable cause of the second outbreak. He had noticed that the disease recommenced after some days rain, succeeded by others of bright sunshine.

“From Bel Etang we went to Mare Jackos to see some graves which were at short distances from the houses, covered with loose stones (some were probably superficial).”

At the bottom of the Mare we found the bed of the River Coignard quite dry. A small canal, with muddy water, was running about twenty yards from it.

Our last visit was to the cottages, apparently in a most healthy

locality, freely exposed to the sea breezes, being at the top of a slope, commanding a view of the sea. Everything showed that the inhabitants had taken pride in the neatness of their property. Yet in these two houses there have been five deaths, and they are now deserted. The graves are in the lower part of the garden, on the sea slope. There is no spring, and *all the water is brought from the canal.*

Articles of bedding were lying exposed in the garden, which we ordered to be burnt, &c., &c.

Next day we visited the Riviere Leche and the estate of Clemencia, marked in the map Hermitage, where of late there has been a great mortality.

The situation is high up, between the Montayne Blanche and the Montayne Fayence, and, as the crow flies about two miles from Mare Jackos.

About 300 yards from the sugar-mill of Clemencia, we stopped at a closed shop, near which are several cottages of Creoles, from whose account we got as follows:—

During the first epidemic, about four months since, two persons, one of whom was the owner of the shop, had died of cholera, since when it had remained closed. On the 13th of this month (November) Madame Alfred, sister-in-law of the former owner of the shop, wishing to know if her shawl was in the house, had a window shutter opened, put her head through the window, and saw her shawl on the floor. She did not remove it, but remarked, on withdrawing her head from the room, that she was “*frappée par la mauvaise odeur.*” The same night she had looseness in the bowels. She went into the woods next day, but during that night became worse, and next evening cholera had fairly declared itself. She died on the 17th. A child whom she had adopted, aged two, was taken ill the same evening, and died the same night.

Another child, Léonore, whose mother had been in constant attendance on Madame Alfred, living within fifteen yards of her, was taken ill at the same time, and died on the 18th. On the same day a carpenter, who lives at a short distance, at the foot of the Montayne Blanche, came here to make a coffin for Léonore; he was seized with cholera on the spot, and died the same night.

On Sunday, the 20th, another Creole, Augustin, whose house was pointed out to us, was taken ill, on return from Mass, and died the same night.

The father of the child Léonore was seen by us in bed, having had an attack of cholera, characterized by all the usual symptoms. His eyes were still sunk, and his face expressive of the cholera state.

From this we proceeded to Clemencia Estate, where there are 458 persons employed. Here cholera appeared among the Indians, about

the 20th November, on which date the first fatal case occurred. Up to November 29th there have been seventy-eight cases more or less grave; of these seventeen have died, and about twenty-five are now convalescent, who have had severe attacks; the remainder were trivial cases. The deaths have been entirely among Indians. To-day many are absent from fear.

The situation of this estate, and of the Creole houses before mentioned, is at the head of the Rivière Lèche. There is a quantity of swampy land at the bottom of the valley. The Creoles informed us that they drank spring water. The manager here tells us that there is a good spring at the side of the hill, not far from the camp, but thinks that the Indians almost exclusively use the water from the marsh. He has formed no opinion as to the cause of this new epidemic; he says that there is constant communication between the Indians of the estate and the Creoles, whose houses we had just left. They go to shops close to that spot.

On referring to the statistics of the first epidemic in the report, I find that here, up to the end of the first epidemic, there had been 120 cases of cholera (in a population of 725), which, I presume, was choleraic diarrhea mostly, as no fatal case is recorded. The date of the first case was June 22.

It is therefore clear that the ground round about must have been largely contaminated by choleraic discharges, which, in a time of drought, would seriously contaminate the soil and subsoil water.

We came to the following conclusions on the questions propounded by the Government:—

1. That the disease as now existing is in all respects the same as the cholera on its first appearance in the island.

2. The chief places where it has of late prevailed are the Camp de Masque, on both sides of the road, particularly at Mon Reve and Bon Espon, on the right hand, and at the Mare Jackos and Queen Victoria on the left. Latterly it has prevailed, with great intensity, at the head of the Rivière Lèche, between the west ends of the Montaynes Blanche and Fayence. It is to be noticed that more than ten days before fatal cases of cholera reappeared in the Camp de Masque there had been fatal cases registered at Frois Islots and Plaine Larcher. According to the list furnished to Dr. Hendée, from the office of the district magistrate, it seems that no death is reported between the 21st September, when one occurred in the Camp de Masque, and the 16th October, when one is recorded at Trois Islots, followed by two others in the same locality on the 20th. These cases were verified by Dr. Guiot. On the other side of the district, on the 18th, the fatal case at Plaine Larcher occurred, ten days after which the first fatal case is registered from the Camp de Masque. The only connexion, by intercourse of infected people, is that

of a child, "Riche," who had been staying at Plaine Larcher for some days, and on his return to the Camp de Masque died of cholera. His name is not in the magistrate's list, and his father could not give in the date of his death.

3 and 4. We have not succeeded in tracing the disease in the Camp de Masque back to any origin external to it. On either side of it sporadic cases were occurring at a time when, judging solely from the bills of mortality, it seems to have wholly ceased there.

When once returned there seems to be proofs of transmission from one part to another by intercourse.

It may fairly be doubted if the disease has ever been extinct in the district since its introduction.

The soil in the Camp de Masque has of late been dry to the last degree; and there is always good natural drainage, except in the course of the River Coignard, at the bottom of Mare Jackos. There is everywhere in it, at present, a complaint of scarcity of water, which has coincided very nearly with the reappearance of the disease. We do not, however, see any reason for associating the two in the relation of cause and effect.

The district about Hermitage is always marshy. We have mentioned Mr. Fonteney's idea of careless interments, but there has been no evidence to show that they have been superficial.—(P. 113).

In connexion with this account of a secondary epidemic, I may bring forward the influence of water supply upon the disease in Flacq and in other districts.

Generally the estates along the coast, from Flacq to Grand River, south-east, suffered little, compared with those on the high road, several of which suffered very severely; at La Caroline out of 410, 146 were attacked, and 115 died.

One part of the high road (from Port Louis to Flacq) enjoyed a marked exemption, viz., that between the fifteenth and eighteenth mile stones from Port Louis. It is remarkable that in this part there are no running streams from which drinking water could be taken; the water being drawn from wells; and a similar piece of good fortune was felt at Samarre, a hamlet opposite the fourteenth mile stone, where a number of the African race were crowded together, and where the epidemic was expected to rage with violence.—(P. 17, Report, 1854).

In the report on the district of Pamplémousses the Government medical officer reports, that "along the banks of rivers and canals it was very virulent, and the victims have been very numerous in the neighbourhood of ill ventilated and badly drained places. All the borderers of the Callebasse River and the old Bois Rouge Canal suffered."—(P. 17, *idem*).

Dr. Clerehew, in the report of the epidemic of 1856, says of this district:—"The mortality has been very great, particularly, as in 1854, along the borders of the canals that exist in this district."—(P. 104 of 1856).

In the Rivière de Rampart district Dr. Gorelay observes, that "from the commencement of 1854 disease of the intestines and diarrhea were very prevalent at the Piton, which he attributes to the use of the water of Rivière de Rampart Canal, which crosses several estates before arriving at the Piton, and the quality of which is deteriorated by the detritus and other impurities with which it is charged; to this cause he further attributes the persistence and intensity of the epidemic in this part of the district."—(P. 121).

In the district of Grand Port the report says:—"The usual immunity from disease in those parts of the district in which there exists a scarcity of running water was observed."—(P. 19).

Local conditions of bad sanitation were not always found coincident with the ravages of cholera.

"Parts of the town of Port Louis, where putrid emanations were most abundant, and the houses situated in the most unhealthy places, suffered less than others, where the air was less tainted and the houses better cared for.

"The Arabs and Malabars in the Bazaar were crowded together in the same room with various animals, and always with quantities of perishable goods, often in a semi-putrid state. The horrible state of these lodgings was known, and dire results were anticipated; contrary to which, the epidemic was less virulent and less fatal than in the most airy and cleanly parts of the town."—(P. 9). A probable solution of this is, that this population was one already acclimatized to cholera influences.

"Another remarkable fact is, that the persons whose business exposed them to daily encounters with the most poisonous emanations did not furnish a corresponding increase of cases of cholera. The Inodore Establishment employs 150 Indians, of whom 90 are constantly collecting the refuse of the streets in the scavenger carts, or in going round at night collecting night soil. Of these 90 men, 15 were taken ill, and 15 died. The director of these establishments attributes these casualties, not to the exhalations from the soil, but to the fact of these men having been obliged to penetrate into the yards of the houses, and to their having carried away with them clothes which had been worn by persons dead of the epidemic, and thrown on the dunghill by their relatives. Of the remaining 60, who were employed in the manufacture of manure from the soil, only two were attacked, who were old men, worn out, and pre-disposed to suffer from any epidemic.

"From twelve to eighteen grave-diggers were employed daily by the municipality; of these men, who pursued their avocations from morning to night, under most distressing circumstances, during more than two months, only two fell victims to cholera."—(Page 9).

Exemptions among Charcoal Burners and Chinese.—"Near Grand River there was an Indian camp—a number of earthen hovels with thatched

roofs; the doors so low that one must stoop to enter; and the interior seldom high enough to allow a full-grown man to stand upright; no window nor aperture, save these doors, no ventilation, no light, no sunshine. The ravages of cholera might have been calculated to reach their acme in this unwholesome spot. On the contrary, fewer persons were affected here than in the neighbouring high ground, or in the most favourably situated parts of Port Louis. Perhaps this immunity may be in some measure ascribed to the fact of about a 100 of these Indians being charcoal burners; for it is ascertained that not one of these suffered from cholera, nor did the epidemic once come into the houses in Port Louis where depots of charcoal were kept.”—(P. 10).

Effects of a strict Cordon Sanitaire.—“A strict cordon was kept on the Civil Prison at Flacq, in which there were upwards of 100 persons, and no case occurred.

Mr. Mintocahio isolated Belle-marre by a strict cordon, and escaped altogether the visitation of the cholera.”—(Page 17).

The Chinese enjoyed an almost entire immunity from mortality—two in 1,800 men. A Chinese doctor informed us “that they seldom drank anything but tea (the equivalent at least of boiled water); that they eat animal food freely, and pork in particular, and a daily preparation, or paste, from shell fish, imported from China; that they smoked a vast quantity of opium; and that this was the great preservative, secret, and shield.”—(P. 10).

Their sanitary state, generally, was as bad as was to be found in the island, as regards their dwellings and habits.

The African race, on the other hand, showed a special proclivity to the disease, and, as before seen, a triple ratio of mortality compared with the general and Indian population.

The cause of this may be a greater susceptibility of race, and that absence of prior cholera acclimatization. They were virgin soil for cholera germs.

That there is an acclimatization to cholera is, I think, very probable. Though there is much to balance in point of time, during which the two epidemics existed in 1854 and 1856, the difference of mortality is very marked:—

In Port Louis, 1854, ...	3,492.
„ 1856, ...	1,397.
In the island at large, 1854, . . .	7,650.
„ 1856, ...	3,223.

The mortality of the garrison in 1856 was thirty-five, having been twenty-eight in 1854. But Dr. Clerchew, the principal medical officer, remarks:—“The soldiers who have recently arrived, and were not present during the epidemic of 1854, form but a small part of the garrison of Port Louis; yet these men have furnished about three-fourths of the military victims of the present epidemic.

In both epidemics the attendants of the Civil Hospital escaped cholera almost wholly. The sick in hospital were equally fortunate.

The epidemic of 1856 repeats the story of introduction from without by ships from India. The *Hyderre* arrived January 5th, and the *Futty Mumbarruck* on the 8th, both with a high mortality and sick list (747 embarked; 55 died on board).

They were put into quarantine of observation until the 12th, when they sailed to disembark the coolies at Gabriel Island, where cholera broke out almost immediately, most probably from the immigrants opening out infected articles; for they were virtually, at first, in the open air, without tents. There is no water on the island. On the 17th, three days after their landing, the medical man sent to take charge of them found two cases of cholera. The disease spread rapidly, proving itself a fresh epidemic, by its ravages. 271, of 692 landed, died on the island, of cholera and other diseases. 83, only, were admitted to be of cholera, which may be doubled; the rest of the mortality was ascribed to dysentery, fever, &c.

The quarantine arrangements were very defective, and were notoriously violated in several instances—*e.g.* :—

1. Two persons were sent from Flat Island to Mauritius during quarantine (Flat adjoins Gabriel Island and furnishes it with water).

2. A workman left Flat Island for Grand Bay, and thence to Port Louis.

3. The effects of a French surgeon were sent from Flat Island to Port Louis.

A fatal case of cholera, the first of the island mortality, took place on board a steamer, which had gone to the islands with stores, on the 26th February. He had been employed in landing provisions. The steamer came into harbour as if nothing had happened. The crew (40) dispersed through the town, though put into quarantine next day; and a few days later—March 3rd—cholera began on the island, and was epidemic from the 12th.

The progress in the districts is not given with the same detail as in 1854, but at p. 104 I find Dr. Clerchew stating that :—“A part of the district of Savanne was affected at the very beginning of the epidemic, the disease being evidently introduced into the district by a prisoner who went down from Port Louis, and was seized with cholera on the road. On his arrival in Savanne the disease spread to those in contact with him.”

He also says :—“Several of the public establishments which suffered most severely from the disease in 1854, have, on this occasion, almost escaped. For example, the prison of Port Louis, which was, in 1854, the principal focus of the disease, has been, on this occasion, almost free from it. This may be partly attributable to the night soil being now

removed by the Inodore Company, and not, as formerly, by the sewers, which are now clean and empty.”

The facts which I have adduced tend to give support to the following views :—

1. That cholera is not due to atmospheric or telluric influences, nor susceptible of spontaneous origin from bad local sanitary conditions.

2. But is due to a contagion, transportable from place to place, namely, by human intercourse, or by clothing, or other articles infected by persons ill of the disease.

3. That when the disease appears in a vessel some time after leaving port, it is probable that the contagion has arisen from exposing some articles of clothing, and which had previously been kept closed from the air.

4. That personal infection takes place under favourable conditions of overcrowding, and with a very short period of latency, and may be presumed to be most common mode when the disease appears on embarkation, or immediately after.

5. That the running water seems to be a powerful agent in carrying the contagion of disease.

6. That drought following cholera may, by contaminating the subsoil water, give rise to secondary epidemics.

7. That previous exposure to cholera influence seems to diminish susceptibility to the disease.

8. That race, under otherwise similar conditions, shows a varying susceptibility, which may, perhaps, be the result, partially, of No. 7.—
October 24, 1866.

Contributions to the History of the Blood Corpuscles. By J. G. VAN DER LITH, Med. Doctorandus. Translated from the *Nederlandsch Archief voor Genees- en Natuurkunde*. Deel II., 2^e Aflevering. Utrecht, 1866. P. 186. By WILLIAM DANIEL MOORE, M.D., Dub. et Cantab; M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

IN the Summer of 1864 I made some investigations respecting the corpuscles of the blood. Although I had not the opportunity, during the present Summer, of continuing these investigations, it seems not inappropriate now to make known some of their results. But observations on this subject, examined by so many, yet still but little known, now rise again from various quarters. May the veil which still covers so much be raised. Perhaps the following may contribute something to this end.

I. *Cell-forms in the Blood of the Adult Frog.*—The development of the blood corpuscles in the adult frog is a much-disputed point. Although it is at present tolerably generally admitted that the lymph corpuscles are converted into blood corpuscles, there is still diversity of opinion as to the intermediate forms. In proof of this I may mention two observers, Rindfleisch and Kneuttinger, both of whom have recently dealt with this question. The former assumes a gradual growth of the lymph corpuscles, and sees the transition forms without trouble;^a the latter finds in all other cell-forms the transition states, and calls the observation of them very difficult.^b The cell-forms indicated by Kneuttinger as such, are, however, by Donders and Moleschott considered as blood corpuscles in process of solution;^c while, finally, Henle cannot find the intermediate forms observed by Rindfleisch.^d Kneuttinger does not even speak of the possibility of Donders and Moleschott being right, as he simply does not mention the idea of these physiologists: I am not aware that it has been refuted. My object is to direct attention afresh to this theory, before doing which it will be well to recapitulate the cell forms which have been observed.

The blood may be considered as a tissue, with movable fundamental forms. This mobility is the cause why forms of all periods are mixed with one another, so that their position does not, as elsewhere, afford an indication of the course of development.

It is, therefore, not surprising that the explanation of the cell-forms met with has been different.

A. *Red Blood Corpuscles.*—Although this term comprises too much, and is so far incorrect, it is difficult to find another. I refer to the red blood corpuscles, κατ' ἐξοχήν, which constitute by far the greatest number of the cell-forms occurring in the blood; only a very small proportion of red corpuscles belong to a kind to be mentioned presently. Those which now occupy us are elliptical, flat, and present a depression on both surfaces. Their magnitude is, according to Welcker: length, 22·3 mmm.;^e breadth, 15·7 mmm.; thickness, 3·6 mmm. Their colour is bright red. No nucleus is distinguishable in blood corpuscles examined immediately after they are taken from the animal, but after some time a nucleus exhibits itself, and is then elliptical. The existence of a membrane is admitted by many and denied by many. No granules are perceptible in the cell.

^a Experimental-Studien über die Histologie des Blutes, 1863, p. 1.

^b Zur Histologie des Blutes, 1865, p. 10.

^c Holländische Beiträge, 1848, i., 368.

^d Jahresbericht für 1863.

^e The reader will please observe that the letters mmm. signify the millionth part of the metre, or the thousandth part of the millimetre. This unit is usually employed by writers on physical optics, and in designating microscopic measurements.—*Translator.*

B. Colourless Blood Corpuscles.—This name has given rise to much confusion, totally different cell forms, which agreed only in the absence of a definite colour, having been indicated by it. Henle made a distinction by separating the fat globules (chyle granules), free nuclei, and immature cells from the lymph corpuscles.^a Wharton Jones went farther, by again dividing the lymph corpuscles: the most recent investigations have shown the correctness of this. We must, in the frog, distinguish the cell forms which have been designated colourless, as—

1. *Lymph Corpuscles.*—These are the amyloid corpuscles of Rindfleisch, the finely granular cells of Wharton Jones,^b the greater part of the granular cells of Donders and Moleschott. These cells get into the circulation from the lymphatic system; their number is tolerably great. Donders and Moleschott found that in eight red blood corpuscles one of these cells occurs,^c but they do not yet distinguish the granular cell from the lymph corpuscles. Rindfleisch obtains about the same result;^d in 240 red corpuscles he finds 30 colourless; 26 of these last belong to the granular cells of Donders and Moleschott, so that the proportion is 1:9; for the amyloid it is 1:10. According to Kneuttinger,^e Rindfleisch found the proportion to be as 1:8. On what grounds he asserts this I cannot see: the magnitude of these corpuscles is about 8 mmm. Their appearance is turbid, as it were cloudy, without distinct separate granules. The form of the cells, when lying at rest, is somewhat elliptical. They occur, for the most part, coherent in little heaps. No membrane is present; little molecules are crowded within the corpuscles. A nucleus becomes evident on the addition of acetic acid; it fills nearly the whole cell; sometimes a few little granules are met with. In all movements are observable, not unlike those of the rhizopodes (amœba), whence the name.

2. *Granular Cells.*—This name, given by Rindfleisch, is, perhaps, less happy on account of the different ideas attached to it. They are the coarsely-granular cells of Wharton Jones, a small section of the granular cells of Donders and Moleschott, the granular cells of Moleschott.^f Ecker does not distinguish these in the frog, although he represents them in some vertebrate animals;^g his cell with granules,^h as well as those of Kölliker and Preyer,ⁱ is a form which is not met with in normal blood. Rindfleisch finds 2 of these in 240 coloured cells. Their magnitude is 12-14 mmm.; they are filled with a number of granules. Their form is round, with sharp contours, which in particular at once distinguishes these from the former ones; they occur in an isolated state. There is no

^a Anatomie, p. 471.

^b Philosophical Transactions of the Royal Society of London, 1846, i., p. 68.

^c Holl. Beitr., i., 367.

^d Zur Histologie des Blutes, p. 10.

^e Experimental Studien, p. 21.

^f Müller's Archiv, p. 13.

^g Icones iii., Fig. ix. d., Fig. x. d.

^h Icones vi., Fig. xi. a.

ⁱ Virchow's Archiv, xxx., Tab. xv., Fig. 5.

membrane. Acetic acid brings one or more nuclei into view; these fill only a part of the cell. The movements, which are observed also in these, are much more extensive than in the above. Valentin's amœba in the blood is a moving granular cell.^a

In frogs, kept long without food, we find both smaller granular cells and cells agreeing in magnitude with granular cells, but otherwise presenting the appearance of lymph corpuscles; it is then very difficult to distinguish them. The difference in movement of the two sorts of cells, such as Wharton Jones has already described, does not exist in the forms mentioned. This would lead one to suspect that the difference in the extent of the movement is dependent on the magnitude of the part of the cell existing around the nucleus—in other words, on the size of the protoplasm.

Ecker distinguishes, as I have already said, the granular cell in other vertebrate animals with nucleated red-blood corpuscles. Leydig^b distinguishes these decidedly from the lymph corpuscles in the blood of the selachii; Erb makes the same distinction in the hen.^c The latter I can confirm; and I may add, that, in the embryos of fowl of about fourteen days, both cell-forms, lymph corpuscle and granular cell, have been seen by me. Remak saw the latter cell in an embryo of sixteen days, without making the distinction mentioned.^d

3. *Free Nuclei.*—These are distinguished by Henle from the colourless, that is, the lymph corpuscles.^e Wharton Jones saw these, but he did not look upon them as free nuclei. Donders and Moleschott found the free nuclei in the blood of frogs kept for a long time without food, but none in that of those that were well fed.^f

Rindfleisch believes that these occur constantly, which Kneuttinger also considers proved. Ecker assumes the existence of the nuclei, but those he calls such are lymph corpuscles; in the frog he represents none. It is not difficult to observe their occurrence in fasting frogs. Their form is round, with a sharp contour; their magnitude is less than that of the lymph corpuscles. Their appearance is dark cloudy. Most of them remain unchanged on the addition of acetic acid; only in some does a substance become visible.^g Wharton Jones also represents something like the nucleus. It is not always possible to distinguish these from lymph corpuscles at rest or dead.

C. Nucleated Cells.—Under the name of pale nucleated cells, Donders and Moleschott, in their frequently-quoted investigations, directed attention to faintly-coloured and partly colourless blood corpuscles; to them they refer a series of cells which are arranged by others under the head both

^a Müller's Archiv, 1841, p. 435.

^c Virchow's Archiv, xxxiv., p. 146. Anatomie, p. 470.

^g Rindfleisch, Exp. Studien, p. 23.

^b Vergleichende Histiol., p. 449.

^d Müller's Arch., 1858, Pl. viii., Fig. 13 x.

^f Phil. Trans., Pl. i., Frog, Fig. 3.

of coloured and colourless cells. The name nucleated cells expresses the character of this group; some are as much coloured as the red-blood corpuscles; these we can, therefore, not call pale. Each of these cells possesses a nucleus which in the blood just taken from the animal is distinctly visible, and has a round form. As even those who do not admit the existence of a nucleus in the red corpuscles cannot deny its presence in this group, the name need not be rejected by them. But that we have no right from these cells to infer the existence of a nucleus in the red cells, as Kneuttinger does,^a is evident, for they are cells of another period. From Kneuttinger's point of view they are young cells; the nucleus might then, just as in epidermis cells, disappear.

To this group belong the colourless nucleated cells and the transitions between these and the coloured nucleated cells of Wharton Jones. The nucleus is round, as large as the free nucleus. The following cell-forms must be distinguished:—

1. *Red Nucleated Cells*.—Under the name of “young blood corpuscle,” Ecker represents an oval corpuscle,^b with normal red colour and round nucleus, in which water is said to produce a granular precipitate. These blood corpuscles had been before spoken of, but Ecker has more definitely directed attention to them. Nasse knew these cells.^c Preyer also announced their occurrence.^d These cells have been by many brought forward as proofs that the nucleus of the blood corpuscles actually exists, as they observed in them a nucleus in the blood while still circulating, as Wedemeyer,^e H. Meyer,^f Kölliker,^g and, recently, Kneuttinger;^h for Wedemeyer, however, who first doubted the existence of the nucleus, this proof is not convincing. Beale's old red blood corpuscles appear also to belong to this category.ⁱ

The name young blood corpuscles is not happy, as it answers the question, what these forms are; and it is the less happy, as the answer is probably entirely incorrect.

Preyer^k saw motor phenomena in these cells in extravasations, in which they are very numerous; these movements consisted in constant cutting off of round particles of various sizes, which finally caused the annihilation of the cell. We not unfrequently see a change in the form of these cells in circulating blood brought under the microscope.

2. *Colourless and Faintly-coloured Nucleated Cells*.—These are round nuclei, surrounded by a pale, scarcely visible oval, sometimes round contour. The contour of the faintly-coloured cells is easily perceptible.

^a *Op. cit.*, p. 9.

^b *Icones*, Tab. iii., Fig. xi.

^c Wagner's *Handwörterbuch der Physiologie* i., p. 196.

^d Virchow's *Archiv*, xxx., p. 435.

^e Meckel's *Archiv*, 1828. ^f Müller's *Archiv*, 1843, p. 208.

^g *Microscopische Anatomie*, ii., p. 583. ^h *Op. cit.*, p. 9.

ⁱ *Medical Times and Gazette*, 1865, April, p. 380.

^k *Op. cit.*, p. 426.

Wharton Jones first described these. Donders and Moleschott, and subsequently Cramer,^a confirmed their existence. Rindfleisch did not observe these cells in the frog.^b In the triton they were, according to him, tolerably numerous. They are very numerous in frogs who have fasted long; at first only the nuclei are seen; after some moments, however, the cell contours become distinct.

3. *Pale Blood Corpuscles.*—Under this name Hensen^c directs attention to faintly-coloured blood cells, which were by him considered to be the same as the “nucleated cells, uncoloured stage” of Wharton Jones, as the pale nucleated cells of Donders and Moleschott, and as the young blood corpuscles of Ecker. But this is incorrect. Even the name would be in all cases badly chosen: thus the first-named are colourless, the last-named are almost as highly coloured as the normal, while the intermediate cells are both coloured and not coloured. Hensen observed in this cell-form protoplasm rays, which are found around the nucleus; these rays are met with in none of the cell-forms spoken of, but are found in blood corpuscles, which are of a very pale yellow colour, and have notched margins. We find these, although in small number, in starved frogs.

D. Transition forms between Lymph Corpuscles and Red-blood Corpuscles.—Rindfleisch has anew directed attention to these cell-forms. They had been seen before, but had apparently been forgotten. Henle could not see them last year.^d Schultz, Wagner, Nasse,^e H. Müller,^f Frey,^g mention these cells, and probably Henle’s immature blood corpuscles, with pale periphery (rind) closely surrounding the nucleus must also be referred to this category. Beale, too, has recognized and represented definite transition forms.^h

We find these cell-forms only in well-fed frogs, which have spent a very short time—one or two days—in captivity; later they have nearly entirely disappeared. This rapid disappearance, by Rindfleisch especially placed in the foreground, is perhaps the cause why many could not see the cells. In very young cells the nucleus is seen, not in others. It is as if a periphery arises around the lymph corpuscle, which becomes larger and larger, the lymph corpuscle thus appearing as nucleus. This hypothesis, formerly put forward by Nasse,ⁱ and subsequently given up,^j is, as it seems to me, most in accordance with these cell forms.

Let us now consider the hypothesis of Donders and Moleschott. These physiologists examined the blood of frogs which had been kept without

^a Müller’s Archiv, 1848, p. 65.

^b *Op. cit.*, p. 24.

^c Zeitschrift für wissensch. Zoölogie, herausgegeben van v. Siebold und Kölliker xi., p. 264.

^d Jahresbericht für 1863.

^e *Op. cit.*, p. 196.

^f Zeitschrift für rationelle Medicin, 1845, p. 271.

^g Histologie und Histiöchemie, p. 168.

^h *Loc. cit.*, p. 330.

Wagner’s Handwörterbuch der Physiologie, i., p. 196.

^j *Op. cit.*, ii., p. 394.

food for various periods. They found, besides the red blood corpuscles a number of nucleated cells and free nuclei, and the latter especially in greater number the longer the animal had been without food. Therefore they correctly look upon these nucleated cells as blood corpuscles in a state of retrogression, and of which, at last, the periphery disappears, leaving only the nucleus. In this view Rindfleisch also coincides, without mentioning the names of the above two physiologists,^a although he assumes another mode of destruction of the red blood corpuscles.^b The blood corpuscles passing into solution occur in very small number in the well fed frog, in which we meet also with very many forms of development. After some days the last disappear, while the first occur more and more abundantly. Thus Donders and Moleschott found many nucleated cells in frogs which had fasted in the course of the Summer for twenty-two days, very many in frogs in February and March. The nuclei of these cells resemble the free nuclei. The latter are here also much more numerous than in the blood of well-fed frogs. They exhibit no contractility, and are, so far as is known, capable of no further development.

Did we wish to look upon the nucleated cells just described as young blood corpuscles, as has been done by many—Wharton Jones, Ecker, Kneuttinger—we should encounter many difficulties. We cannot assume two wholly different transition forms: if, therefore, we indicate those described under *D* as forms in a state of development, we must ascribe another signification to the nucleated cells. If we further take into account that the nucleated cells occur very sparingly in well-fed animals and very abundantly in the badly-fed, we should, on another hypothesis have the strange phenomenon before us that the development of the cells had taken place more rapidly when food was withheld than when it was copiously supplied. That this does not accord with what we know of the development of the blood corpuscles in the mammalia, nor with the development of tissues in general, needs no proof. In extravasation we see many nucleated cells produced, and in these we will not assume any development of blood corpuscles.

Round, Dark-coloured Blood Corpuscles.—One form of blood corpuscles remains which I have not yet mentioned. As I believe that they do not occur in blood examined immediately after it is drawn from the vessel I mention them after all the others. They have been made more generally known by Donders and Moleschott, who described them as round, glittering, dark-coloured forms, long resisting the action of water.^c Nasse had previously seen them, and given them the name of non-nucleated cells.^d H. Meyer describes, in the triton, small, dark-coloured blood corpuscles, with a nucleus scarcely or not at all visible.^e Whether Ecker

^a *Op. cit.*, p. 24.

^c *Op. cit.*, p. 363.

^d *Op. cit.*, p. 196.

^b *Op. cit.*, p. 42.

^e Müller's Archiv, 1843, p. 206.

has seen these in the spleen, it is, in the different cell-forms mentioned by him, not possible to say with certainty.^a Funke did not find these corpuscles in blood examined immediately, but observed that they form in the preparation under the microscope. He assumes a nucleus in them, for he says that the empty cell envelope remains; the nucleus must, therefore, be concealed in the "cone of colouring matter," as he calls these corpuscles.^b Rindfleisch, in 1863, brought these corpuscles again under notice.^c Preyer^d and Kneuttinger^e also mention them. Preyer differs from most of the writers above named, as he does not observe these corpuscles constantly in each drop of blood. The colour he calls blood-green. Some did not find a nucleus; others did. Preyer, in perfectly analogous forms in extravasated blood, saw that the nucleus was sometimes absent, sometimes present.

Whether these corpuscles occur perhaps in the Spring, after the animals have for some months suffered hunger, I am not aware. Now, in December, after a couple of months fasting, they are not yet met with. But they are produced, as Funke correctly remarked, in drawn blood, under the covering glass. That the cause of the occurrence of these forms is not the access of air nor evaporation of water is proved by the red blood corpuscles, whose nucleus is not yet visible, when these round corpuscles have already arisen.

Donders and Moleschott considered these corpuscles, on account of the absence of the nucleus and their diminution during fasting, as more highly-developed blood corpuscles, as homologues of the blood corpuscles of the mammalia. The later observers above named look upon them, on the contrary, as parts of red blood corpuscles in process of annihilation. In this view we must agree, particularly as these have been found also in extravasated blood, and their origin has been directly observed by Preyer, who saw them dividing into smaller and smaller particles. These round corpuscles possess, however, the power of altering their form, so that they cannot be mere drops of colouring matter, but must possess protoplasm. In frogs which had during the Summer been long kept confined, I observed different changes of form in these corpuscles; long, partly coloured, partly colourless prolongations took place, which were subsequently again drawn in; these prolongations sometimes exceeded the coloured blood corpuscles in length. The want of agreement respecting the presence of the nucleus is strange. Kneuttinger, to whom the results of Rindfleisch, who found a nucleus, were known, found none; and as such observers as Donders and Moleschott could find none, we may safely look upon this as correct. Probably different varieties are still hidden among them.

^a *Zeitschrift für ration. Medicin*, vi., p.

^b *Atlas der phys. Chemie.*, Tab. xii., Fig. 4.

^c *Op. cit.*, p. 2.

^d *Virchow's Archiv*, xxx., p. 425.

^e *Op. cit.*, p. 11.



Fig. 1.



Fig. 2.



MR PORTER ON EXCISION OF THE UPPER JAW.